# Popular Astronomy.

Vol. VII. No. 5. MAY, 1899. Whole No. 65.

## LANE'S LAW OF INCREASE OF TEMPERATURE IN A GASEOUS SPHERE CONTRACTING FROM THE LOSS OF HEAT.

C. A. YOUNG.

FOR POPULAR ASTRONOMY.

Astronomers have been a good deal surprised that Professor See should lay so much stress on his re-discovery of a law that has been known for thirty years, and for more than twenty has been given in the text-books used in our schools and colleges; and still more surprised that he should attach so much importance to deductions from it, as if it could be assumed to represent the actual facts of stellar temperature.

When the law was first announced in 1869, as a sort of "by-product" of Mr. Lane's paper on the equilibrium of the solar atmosphere, it excited a great deal of discussion on account of its apparently paradoxical character. I remember very well my first acquaintance with it in 1870, derived from a conversation with Professor Benjamin Peirce, who was much interested in it, and subsequently published two or three papers on the subject in the Proceedings of the American Academy (Boston). So far as I know, its first printed appearance in a form to be easily recognized was in 1878 in Professor Newcomb's Popular Astronomy, where it may be found on page 520 with a demonstration substantially the same as that given in section II of Professor See's article. It is given both in the original Edition, and in the abridgement which is used in many schools to this day. It appears also in Newcomb and Holden's "College Astronomy"; in my little book "The Sun"; in my "General Astronomy"; in Langley's "New Astronomy"; in Proctor's "New and Old Astronomy"; in the "Astronomy" (by Miss Clerke) of the "Concise Knowledge Library", and in Professor Todd's new School Astronomy;—in fact in nearly all the text-books that have appeared in this country for the last twenty years; though of course not in the shape of a *formula* introducing the idea and symbol of absolute temperature.

It would doubtless, however, have taken a much more conspicuous place in astronomical discussion and speculation, were it not

for the well understood limitations which greatly restrict its application. It assumes that the contracting mass is entirely composed of gas, without any solid or liquid admixture, and that the gas is a "perfect" one,—*i. e.* that it is under such conditions of pressure and temperature that it obeys exactly the two laws of Boyle and Gay Lussac which define the relation of volume to pressure and temperature. Now, under laboratory conditions even, all our known gases are distinctly "imperfect," exhibiting measurable deviations from these laws. In fact, if we accept the kinetic theory of gases, these laws are, and can be, valid only under such circumstances that the free paths of the molecules are *long* as compared with the size of the molecules themselves, and yet *not so long* that the encounters between the molecules become relatively infrequent. A substance, oxygen for instance, possesses the familiar physical properties of a gas, only through a comparatively limited range of pressure and temperature. It must be neither too dense nor too rare; too hot nor too cold.

Nor is it probable that any nebula is entirely gaseous. It is true that the nebula-spectrum is one of bright lines, but that is no evidence of the absence of non-luminous solid or liquid material in notable proportion, and if such material is actually present in any considerable quantity the law is inapplicable.

It is unsafe therefore to adopt this law as expressing quantitatively and exactly the relation between the diameter and temperature of a shrinking nebula, especially after it has become a "star," possessing a photosphere like the Sun's, an envelope of "cloud," made up of solid or liquid particles mingled with gas still uncondensed. The law only serves to indicate *in a general way* what may be expected to happen in a contracting nebula; first, a rise of temperature reaching its maximum some time after a photosphere has been formed; followed by a probably long period of approximate constancy; with, finally, a pretty rapid fall after the photospheric envelope becomes too thick, or dense, or viscous to allow free convection currents between the interior and the surface.

But there is no probability that the formula of Lane's law will give trustworthy numerical results for the temperature of a body resembling our Sun.

A word should be said with respect to paragraph VIII of Professor See's paper. In this paragraph he implicitly assumes that the radiating power of the Sun's surface is directly proportional to its temperature; arguing that "as the size of the disc is proportional to the square of the Sun's radius, while the tempera-

ture is inversely as the radius, it follows that the heat received by the Earth will experience a secular diminution proportional to the contraction of the Sun's radius". Now, while we do not yet know certainly the exact law which connects the absolute temperature of a surface like that of the Sun with its radiating power, there is no question that the latter increases much faster than the temperature:—according to Stefan the radiating power is proportional to the *fourth* power of the temperature, while Rosetti makes it more nearly proportional to the *third*. According to either of these formulae the amount of heat received by the Earth, instead of diminishing, must continually increase as the Sun's diameter grows smaller; assuming, of course (what is not true), that Lane's law of temperature holds good throughout the process.

PRINCETON, N. J.,
April 22d, 1899.

## PERTURBATIONS OF THE LEONIDS.

G. JOHNSTONE STONEY AND A. M. W. DOWNING.*

When the present investigation was undertaken, our knowledge of the perturbations of the Leonids was due to an investigation carried on thirty years ago by Professor J. C. Adams.†

His object was to compute the shift in the nodes of the meteoric orbit due to perturbations, and to compare the calculated amount with the amount which had been deduced by Professor Hubert A. Newton from observations made at intervals during the last 1000 years.‡

For Professor Adams' purpose the perturbations to be computed were the average perturbations; and he accordingly employed Gauss' method, in which the mass of the disturbing planet is supposed to be distributed round its orbit in quantities proportional to the time that the planet occupies in travelling over each portion of its course. This elegant method furnishes the average amount of each perturbation on the supposition that the periodic times of the disturbed body and of the disturbing planet are incommensurable, so that in the course of

* Paper read before the Royal Society 1899 March 2.

† "*Comptes Rendus*", March 25, 1867, p. 651; and for a fuller account see "*Monthly Notices of the Roy. Astron. Soc.*" April, 1867, p. 247; or "*Monthly Notices*", March, 1897, p. 387, where the last-mentioned paper is reprinted.

‡ "*Silliman's Journal*" 1864, vol. 37, p. 377; and vol. 38, p. 53.

time the two bodies present themselves in every possible position to one another.

This condition, however, has been but imperfectly fulfilled within the limited period of 1000 years over which the recorded observations extend, especially in the case of the three planets which influence the Leonids most, and indeed are almost the only planets whose attraction needs to be taken into account. These are Jupiter, Saturn and Uranus. A comparison of the periodic times shows that fourteen revolutions of Jupiter approximate in duration within about one-fifth of a year, to five revolutions of the meteors; two revolutions of Uranus occupy about one and three-quarters of a year more than this same time, and nine revolutions of Saturn correspond within a fraction of a year to eight revolutions of the meteors.

These cycles have been several times repeated within the period over which the observations extend; and one consequence of these cycles is that there have been oscillations in the rate of the advance of the node about its mean value, so that the times for the showers assigned by applying to the orbit the average shift of its node, have usually differed by several hours from the actual times. On one occasion—in A. D. 1533—the shower anticipated the computed time by about twenty-six hours, and, as the present investigation shows, a deviation of comparable amount and in the opposite direction is to be expected this year. Accordingly, even if our sole object were to enable astronomers in future to predict more satisfactorily the times of the greater Leonid Showers, it would be necessary to prepare for the task by first studying the actual amount of the perturbations in each revolution, and moreover, for meteors occupying various stations along the stream.

For, in fact, the perturbations have not only differed in different revolutions, but even within a single revolution, the meteors which occupy successive positions in the procession are differently affected by the surrounding planets, as is confirmed by the definite results which Herr Berberich has obtained by assuming successively two epochs for the perihelion passage.* The dense part of the stream, with which we are chiefly concerned, and

* See his paper on the perturbations since 1890 of the orbit of the comet which is associated with the Leonids, *Astr. Nachr.*, No. 3526.

which we may call the ortho-stream,* is now so long that it takes between two or three years to pass each point in its orbit, so that the configurations in which the several parts are presented to the disturbing planets are markedly different. Accordingly, perturbations must have produced in this long stream both sinuosities and an unequal distribution of density;† and the first step towards increasing our acquaintance with these and other kindred phenomena, as well as towards gaining a better insight into the past history of the swarm, is to aim first at securing a more intimate knowledge of the perturbations.

With this end in view it was decided, as a first step, to compute the actual perturbations of a definite part of the stream over the whole of one revolution, taking that part of the ortho-stream of which Adams had determined the orbit, and extending the computation over the revolution from the date of the great shower of November, 1866, until that day in January, 1900, when the same part of the stream will return to the Earth's orbit.

Adams' calculation was based on determinations of the radiant point which were made in 1866, before photography had lent the aid to astronomy that it now yields. Moreover, the circumstance that the Earth deflected the meteors which were then observed by an amount which varied as the shower progressed, was not at that time attended to by observers. Owing to these imperfections, there is a considerable probable error in the mean of the determinations which were made in 1866, and a corresponding uncertainty in the values of the elements computed from that mean. We are accordingly only justified in employing Adams' orbit as approximate. But, fortunately, an error in the orbit, of such an amount as is at all likely to exist, will not materially affect the perturbations of the orbit, which are what we have at present in view.

* In order to facilitate the study of the Leonids it is convenient to distinguish between a great body of them—the ortho-Leonids—which are travelling round the Sun in nearly identical orbits, and another class of Leonids which we may call clino-Leonids that are pursuing courses which differ in a more considerable degree from the ortho-orbit. By the ortho-orbit is to be understood the mean of the orbits of the ortho-Leonids.

The ortho-Leonids at present form a compact stream of such a length that it takes nearly three years to pass each point of its orbit, and so narrow that when the Earth passes obliquely through it the transit occupies only some five or six hours; whereas the clino-Leonids form a less dense and wider stream, which has spread itself the whole way round the ring, and which produces in every November, when the Earth passes through it, a feeble meteoric shower that lasts for several days.

† One consequence of the existence of irregularities of the stream of ortho-Leonids is that the ortho-orbit at one cross-section of the stream (*i. e.*, the mean of the orbits of the meteors occupying that situation in the stream) is in general not absolutely identical with the ortho-orbits at other cross sections.

The main stream of Leonids—the ortho-stream—is narrow and very long, and it is convenient to divide it into segments, each of which shall be of moderate length. Through one of these, which we may call segment A, the Earth passed in November, 1866, and on that occasion there was withdrawn from it that small portion which consisted of meteors which either encountered or passed close to the Earth. Those that actually plunged into the Earth's atmosphere were destroyed: those that passed near were deflected, and were also either accelerated or retarded, and they thus became clino-Leonids. It is with the great majority of the meteors in segment A, which escaped both these fates and continued to be ortho-Leonids, that Adams' investigation is concerned. He ascertained their orbit; and starting from the elements of the orbit as determined by him, the actual perturbations which it has since undergone have been computed, and the main results thus arrived at are embodied in the following table.

PERTURBATIONS OF THE ELEMENTS OF THE ORBIT OF SEGMENT A OF THE ORTHO-STREAM IN CERTAIN SELECTED INTERVALS OF TIME.

The Elements are referred to the mean Equinoxes of their respective epochs.

| Elements. | | Elements of the osculating ellipse on 1866, Nov. 13*d* 13*h*, as found by Adams. | Perturbations of the elements in the selected intervals. | | | | | Computed values of the elements on 1900 Jan. 27*d* 15*h* |
|---|---|---|---|---|---|---|---|---|
| | | | I | II | III | IV | V | |
| | | ° ′ | ′ | ′ | ′ | ′ | ′ | ° ′ |
| Mean longitude in orbit | $\varepsilon$ | 58 10.2 | − 4.83 | − 0.32 | − 27.98 | − 13.99 | − 0.70 | 58 34.4 |
| Longitude of perihelion | $\pi$ | 58 19 | − 5.37 | + 10.70 | − 6.47 | − 4.75 | − 0.60 | 58 40.6 |
| Long. of node (descending) | $\nu$ | 51 28 | + 29.33 | + 7.15 | + 1.69 | + 70.83 | + 0.09 | 53 41.8 |
| Inclination | $i$ | 16 46 | + 11.92 | − 1.01 | − 1.43 | − 28.60 | − 0.01 | 16 29.7 |
| Angle of Eccentricity | $\varphi$ | 64 46.8 | − 3.39 | − 1.70 | + 12.06 | + 7.65 | + 0.32 | 65 1.7 |
| Mean distance | $a$ | 10.3402 | + 0.015660 | − 0.021271 | + 0.033726 | + 0.038258 | + 0.001747 | 10.40832 |
| Daily motion of | $n$ | − 1°.77857 | + 0.″004069 | − 0″.005481 | + 0.″008678 | + 0.″004763 | + 0.″000441 | − 1°.76110 |

I is the interval from 1866, November 13, to 1871, May 3. In this interval segment A of the ortho-stream crossed the orbit of Jupiter and Saturn.

II is the interval from 1871, May 3, to 1894, December 28. In this interval it crossed the orbit of Uranus, both on the outward and homeward journeys.

III is the interval from 1894, December 28, to 1897, December 30. In this interval it recrossed the orbit of Saturn.

IV is the interval from 1897, December 30, to 1899, May 18. In this interval it recrossed the orbit of Jupiter.

V is the interval from 1899, May 18, to 1900, January 27. This interval brings segment A of the stream back to its descending node.

As already stated, the calculation has been extended over an entire revolution of that portion of the stream which we have called segment A; and in computing the perturbations, account has been taken of the attraction exercised upon these meteors by Mars, Jupiter, Saturn and Uranus. At first Venus and the Earth were included, but as the influence of these planets was found to be insensible, they were omitted from the latter part of the calculation.

The expense of carrying on the work has been met partly out of the Government Grant administered by the Royal Society, and partly out of the Royal Society's Donation Fund. The computations have been made by Messrs. F. B. Cooper, J. H. Bell, and W. H. Walmsley, members of the staff of the Nautical Almanac office. We are also indebted to Mr. E. Roberts, the chief assistant, for his aid in various parts of the work. The method adopted was that by mechanical quadratures, the determinations of the variations of the elements being made at intervals of thirty-six days, except for the period from May, 1871, to December, 1894, during which time the perturbations were small and progressed so regularly that it was found sufficient to make the computations at intervals of 216 days.

The most noteworthy features are a near approach to Saturn in April, 1870, and a near approach to Jupiter in August, 1898, at which latter time the meteors in segment A of the stream were at a distance from the planet of only 0.9 of the mean radius of the Earth's orbit, The consequences of these near approaches are brought out in the table. Uranus produced but little effect in this revolution. The planet was at a distance when the swarm crossed his orbit. And the influence of Mars was trifling. So that nearly the whole of the perturbations during this revolution have been caused by Jupiter and Saturn.

The following were the adopted masses of the disturbing planets.

| | |
|---|---|
| Mars | 1:3093500 |
| Jupiter | 1:1047.879 |
| Saturn | 1:3501.6 |
| Uranus | 1:22756 |

In consulting the table, it is to be borne in mind that $\epsilon$, which is there designated, in compliance with the usual convention amongst computers, the "mean longitude in the orbit", is in reality the sum of two angles lying in different planes, viz., the longitude of the node + the angle between the radii from the Sun to the node and to an imaginary body starting from perihelion

at the same epoch as segment A of the meteors, and thenceforward moving uniformly in a circular orbit round the Sun in the same plane and with the same periodic time as the meteors. So again $\pi$, the so-called "longitude of perihelion", is the sum of two angles, viz., the longitude of the node measured along the ecliptic, + the angle from the node to the perihelion measured in the plane of the orbit. The second angle in each case, that in the plane of the orbit, is measured in the direction of positive motion.

The perihelion distance in Adams' orbit, of which the elements are in the first column of the table, and which was the osculating ellipse on 1866, November 13, is 0.9855; that of the osculating ellipse on 1900, January 27, of which the elements are in the last column, is 0.97296. There is a corresponding difference in the distances of the node from the Sun, a difference which would be enough to carry segment A of the meteoric stream inside the Earth's orbit without intersecting it when it passes the Earth's orbit on 1900, January 27, unless the depth of the stream towards the Sun is greater than its width at right angles to that direction—a width which from observation has been estimated to be about 100,000 miles. We have, however, satisfied ourselves, from the dynamical conditions which must have prevailed when the Leonids joined the solar system, that the depth of the stream is much greater than its width.

The longitude of the node at the epoch 1900, Jan. 27, would be 52° 25′, if computed in the way which has been hitherto usual, by applying to the longitude at the time of the shower of 1866 the average apparent shift of the node as determined from observation by Professor Newton, viz., 102″.6 annually; whereas in the orbit of our table it is 53° 42′. It thus appears that the amount of this perturbation upon segment A of the stream has been more than three and a half times its average amount, and, doubtless, the perturbations in this revolution of the other elements have also been excessive as compared with their average amounts. Thus, the mean distance of the meteors occupying segment A of the stream has been undergoing so much extension, that the meteors will at the end of the revolution find themselves with a periodic time longer by one-third of a year—an amount of change which must largely affect their future history, unless this great perturbation is compensated by what happens elsewhere or at other times.

At the epoch 1899, November 15, the longitude of the node will be 53° 41′.7, a position which the Earth will reach on 1899,

November $15^d$ $18^h$. It is probable, therefore, that the middle of the shower of the present year (1899) will occur nearly at this time, since segment A in the stream, for which our calculations have been made, is situated in the stream less than three months' journey of the meteors behind the segment which the Earth will encounter next November, and which we may call segment B. This conclusion, however, rests on two assumptions: (1) That segments A and B were, in 1866, moving in orbits that did not much differ; (2) That the perturbations which segments A and B have since suffered have not much differed. Both assumptions are probable, but unfortunately neither is certain; so that the prediction can only be offered with reservation. If the shower occurs at the time anticipated, it will be visible from both Europe and America.—*Astronomische Nachrichten No. 3555.*

## A NEW SATELLITE OF SATURN.

EDWARD C. PICKERING.

Nearly all of the astronomical discoveries made by the aid of photography have related to the fixed stars. In the study of the members of the solar system, the results obtained by the eye are generally better than those derived from a photograph. For many years it has been supposed that photography might be used for the discovery of new satellites, and in April, 1888, a careful study of the vicinity of the outer planets was made by Professor William H. Pickering. Photographs were taken with the 13 inch Boyden telescope, with exposures of about one hour, and images were obtained of all the satellites of Saturn then known, except Mimas, whose light is obscured by that of its primary. It was then shown that Saturn probably had no satellite, as yet undiscovered, revolving in an orbit outside of that of Enceladus, unless it was more than a magnitude fainter than Hyperion. (Forty-third report, p. 8.)

In planning the Bruce Photographic telescope, a search for distant and faint satellites was regarded as an important part of its work, and accordingly, plates for this purpose were taken at Arequipa, by Dr. Stewart. A careful examination of these plates has been made by Professor William H. Pickering, and by superposing two of them, A 3228 and A 3233, taken August 16 and 18, 1898, with exposures of $120^m$, a faint object was found which appeared in different positions on the two plates. The

same object is shown on two other plates, A 3227 and A 3230, taken August 16 and 17, 1898, with exposures of $60^m$ and $122^m$, respectively. The position is nearly the same on the two plates taken August 16, but on August 17 it followed this position 33″, and was south 19″, while on August 18 it followed 72″, south 43″. Its motion was direct, and less than that of Saturn, though nearly in the same direction. It cannot, therefore, be an asteroid, but must be either a satellite of Saturn or a more distant outside planet. The proximity of Saturn renders the first supposition much more probable. On August 17, the position angle from Saturn was 106°, and the distance 1480″. Assuming that it was at elongation, and that its orbit is circular, its period would be 400 days, or five times that of Iapetus. It was at first identified with a very faint object found on plates taken in 1897, and the period of seventeen months was derived from them. This supposition has not been confirmed.

Measurements of the positions of the images give additional material for determining the form of the orbit. The method of measurement is that described in the Annals, Vol. XXVI, p. 236. The uncorrected positions of the four images referred to the first plate of August 16 as an origin, are for $x$, 0″.0, + 1″.2, + 33″.6, and + 71″.8; for $y$, 0″.0, − 1″.7, − 19″.8, and − 42″.1; the corresponding Greenwich mean times are $12^h\ 16^m$, $14^h\ 18^m$, $12^h\ 56^m$, and $13^h\ 12^m$. Correcting for the motion of Saturn, the relative motion with reference to that body is in $x$, 0″.0, − 2″.4, − 10″.7, and − 22″.0; in $y$, 0″.0, + 0″.1, + 2″.4, and + 2″.9. It appears from this, that the apparent motion is about 10″.4 a day, at a distance of 1480″. A computation shows that if the orbit is circular, the period must be either 4200 or 490 days, according as the satellite is near conjunction or elongation. These values may be greatly altered if the orbit is elliptical. Since the interval of time between the first and last photographs on which the satellite appears is only two days, it is impossible to predict its position with accuracy. It is probable that its position angle from Saturn now lies between 280° and 290° and its distance between 20′ and 30′. These uncertainties will probably be greatly diminished from measures of plates of Saturn taken in Arequipa on September 15, 16 and 17, 1898, which for some unexplained reason have not yet been received in Cambridge.

The direction of the motion, which is nearly towards Saturn, shows that the apparent orbit is a very elongated ellipse, and that it lies nearly in the plane of the ecliptic. Professor Asaph Hall has pointed out that this is to be expected in a body so dis-

tant from Saturn. The attraction of the latter only slightly exceeds that of the Sun. Hyperion appears as a conspicuous object on all four of the plates, and the new satellite appears about a magnitude and a half fainter on each. The approximate magnitude is therefore about 15.5. As seen from Saturn, it would appear as a faint star of about the sixth magnitude. Assuming that its reflecting power is the same as that of Titan, its diameter would be about two hundred miles. It will, therefore, be noticed that while it is probably the faintest body yet found in the solar system, it is also the largest discovered since the inner satellites of Uranus in 1851. The last discovery of a satellite of Saturn was made in September, 1848, by Professor William C. Bond, then Director of this Observasory, and his son, Professor George P. Bond. The satellite Hyperion was seen by the son on September 16 and 18, but its true character was first recognized on September 19, when its position was measured by both father and son (See Annals, II, p. 12). Soon after, it was discovered independently by Lassell at Liverpool.

Professor William H. Pickering, as the discoverer, suggests that the name Phoebe, a sister of Saturn, be given to the new satellite. Three of the satellites, Tethys, Dione and Rhea, have already been named for Saturn's sisters, and two, Hyperion and Iapetus, for his brothers.

HARVARD COLLEGE OBSERVATORY, Circular No. 43.
April 10, 1899.

---

## REMINISCENCES OF A SEARCH FOR "VULCAN" IN 1860.

H. P. TUTTLE.

FOR POPULAR ASTRONOMY.

Many of the readers of POPULAR ASTRONOMY who have reached the age of three score will recall to mind the great excitement created in the astronomical world by the announcement in more than one European Journal that a certain Frenchman, one Dr. Lescarbault, had, late in 1859, observed a transit of a body across the Sun's disc, and announced that it could be nothing less than an inter Mercurial planet; and further, that no less a personage than the great Le Verrier had most thoroughly investigated the Doctor's records and observations and found a period of revolution of the stranger to be about twenty days. This important "news" was copied into several of our great dailies, and one, an eastern paper of large circulation and influ-

ence, soundly berated the Cambridge Observatory for having "permitted an amateur astronomer, armed with a telescope no more powerful than an opera glass, to make this greatest of all modern discoveries in the heavens," and, "these gentlemen at the Cambridge Observatory, having at their command the best instruments in America, seem to have been asleep or too lazy to do anything except to draw their pay!"

Near the close of March, 1860, Professor G. P. Bond, then Director of the above mentioned Observatory, told me that he had just received a letter from Yale College, in which it was stated that the astronomers there had been on the lookout for a transit of the new planet over the Sun's disc, but had met with no success. Professor Bond then asked me if I would not undertake a search for the new planet during the whole month of April with the West Equatorial (a 4-inch telescope), and that if I consented he would relieve me from my duties of comet and planet hunting, but, continued he, "the meteorological observations must be kept up at all hazards." I was only too glad to take up the new work and told that kind-hearted man and great astronomer that nothing, so far as I was concerned, would be left undone in order to carry out his desire.

At that time the best known solar eye-piece was the Hodgson,* and in a few days Alvan Clark had one of these made and put on the equatorial, and the view of the solar surface was pronounced to be "exquisite," as it really was when compared with the rude contrivances before used for more than a century. I began my observations towards the close of March and continued them during the whole month of April. My instructions were to begin my work as soon after sunrise as possible and continue to observe the solar surface twice every hour until nearly sundown. Professor Bond informed me that if I found direct observations too trying to the eye I might, occasionally, project the solar image on a screen; but I found this method more trying to the eye than in using the solar eye-piece, and after a few hours' trial gave it up.

During the month, if I rightly remember, there were eighteen days on which observations were made. Some days were cloudless, but on others the Sun could be observed for a few minutes at a time only but at nearly every hour during the day.

When the end of the month came I reported to the Director that I had seen nothing on the Sun that could possibly have

* I believe the polarizing eye-piece had then been invented but had not come into general use.

been a planet in transit—nothing but real solar spots; but what I did *see* with my right eye whenever I looked at a lighted lamp during the following three months, was all the colors of the solar spectrum! Had I continued these observations for a couple of months longer I should have ruined the sight of my right eye forever, for the reason that after a few days' work I kept experimenting with light green and blue glasses in place of using the neutral tint which I thought was a little too dense to give the best view of the solar surface. At one time I could, with inconvenience, look at the Sun through a solar eye-piece with no other protection to the eye than an ordinary green or blue glass as is found in ordinary shade spectacles.

So far as I know, no systematic observations for the discovery of Lescarbault's "Vulcan" were made in America except at New Haven and Cambridge. The astronomers at Washington, Clinton, (Hamilton College) and Ann Arbor did not give the matter any serious consideration. If other American observatories took a hand in the matter I have failed to see their records.

From what has been discovered in the solar system during the past ten years it would be rash and even foolish for any one to say that there is not one or more inter-Mercurial planets, simply because they have not been *seen* on the solar disc. Our methods of observation, both visually and photographically, must be greatly improved before we can see bodies like the smallest asteroids projected on the solar surface.

SLIGO, Md.,
1899, March 22.

## IS AQUEOUS VAPOR PRESENT ON MARS?

EVERETT I. YOWELL.

FOR POPULAR ASTRONOMY.

While looking over back numbers of POPULAR ASTRONOMY recently, I noticed that no account of the negative view of this question has been given in your valuable publication. Most of the writers, who refer to the subject, hold to the view that water is a prominent constituent of the planet's atmosphere and that the surface phenomena can only be interpreted by its presence. A consideration of the opposite view may be of interest to your readers.

It will be profitable to review briefly the general subject of the

Martian atmosphere* before considering our special question. Of fundamental importance in such consideration is the mass and size of the planet. If we suppose that the different planets started in their evolution with attendant atmospheres roughly proportional to their masses, that of Mars wonld be $\frac{1}{8}$ that of the Earth. As its surface is 0.28 that of the Earth, the amount of atmosphere over each unit of surface would be about ¼ the corresponding amount for the Earth. The absence of prominent twilight effects and the distinctness with which the surface features can always be seen are additional evidences of atmospheric rarity. Spectroscopic evidence led Professor Campbell† to believe that the Martian atmosphere could not be ¼ as extensive as our own.

The temperature of the planet has a most important bearing on the presence of water there. Mars receives less than half (0.43) as much heat from the Sun as the Earth receives. It is probably older than the Earth, and owing to its smaller size, would pass much more rapidly through similar periods of development; hence we may infer that it has lost most of its internal heat and is much colder than the Earth. This, in connection with the fact that a thin atmosphere retains much less heat than a thick one, renders it probable that the temperature on Mars is much lower than on the Earth. As the Martian year is almost twice as long as our year, the corresponding seasonal changes will be greater.

Turning now specifically to our subject, direct evidence of aqueous vapor is lacking. Some observers thought they detected clouds on the surface, but the reliability of such observations is still in dispute; even if we admit the presence of cloud forms, they do not necessarily involve the presence of water. Spectroscopic evidence would be much more satisfactory—but the results obtained are contradictory, for the test is a very delicate one. The spectrum of the Sun, as we see it, consists not only of the true solar spectrum but also of superposed absorptive lines due to our atmosphere. For our purpose we will divide these telluric lines into two groups, those due to aqueous vapor and those due to the rest of the atmosphere. These lines vary in number and intensity with the different thicknesses of atmosphere traversed by the light ray, and with the varying

* For a discussion of the atmosphere, see "Mars—Atmosphere," by Percival Lowell, POPULAR ASTRONOMY, Vol. II, No. 4, and "Concerning an Atmosphere on Mars," by W. W. Campbell, Pub. of Ast. Soc. of Pac., Vol. VI., No. 38.

† Spectrum of Mars, by W. W. Campbell, Pub. of Ast. Soc. of Pac , Vol. VI., No. 37.

amounts of aqueous vapor present at the time of observation. Now as Mars shines by reflected sunlight, its spectrum will consist of the true solar spectrum plus absorptive lines due to its own atmosphere plus the telluric lines. To eliminate from this composite spectrum the true solar and the varying telluric parts, we must examine the solar spectrum at the same time and under the same conditions. The Moon enables us to do this; having no atmosphere of its own, its spectrum consists of that of the Sun with the added telluric lines. Now if we examine the spectra of Mars and the Moon, when the two bodies have the same altitude, any extra lines in the spectrum of Mars will be due to its atmosphere. The earlier spectroscopic observations, indicated the presence of both atmosphere and aqueous vapor; those of Campbell, made under more favorable circumstances indicated neither, though he estimated that an atmosphere one-fourth as extensive as our own could not have escaped detection. With such an atmosphere similar to our own, the climate would be too cold for water to exist as a liquid and the planet would be snowcapped as are our lofty peaks.

The behavior of the polar caps has furnished the strongest argument for the presence of water on our sister planet. These have long been known to change in size, diminishing when turned toward the Sun and increasing when turned away from the Sun. The analogy is striking enough to lead at once to the hypothesis that they are composed of ice, an hypothesis involving the presence of aqueous vapor in the planet's atmosphere. As the thin atmosphere and greater distance of Mars indicate a cold climate, this hypothesis necessitates the further assumption of some inherent peculiarity in the planet's atmosphere that enables it to retain a greater proportion of incident solar heat than our atmosphere does. Aqueous vapor has been suggested as the agent which accomplishes this; but the known tenuity of the atmosphere and the cloudless appearance of the planet leads us to reject the assumption that there can be enough aqueous vapor present so materially to ameliorate the planet's climate.

It is not necessary, however, to make such an assumption, involving a condition not substantiated by other physical facts. The phenomena of the polar caps can be accounted for by the presence of carbon dioxide, a gas twenty-two times as heavy as hydrogen. In our atmosphere the amount of the gas remains almost constant, although it is being increased and decreased by organic agencies. While the amount of gas present is comparatively small, the amounts of carbon and oxygen are such that

under suitable conditions, carbon dioxide might become a prominent atmospheric constituent. It is heavy and sluggish, freezing at — 78° C. (— 109 F.) into a white snow. By assuming the presence of a relatively large quantity of carbon dioxide on Mars, the formation of the polar caps is easily explained. When, during the long winter, the temperature descended below — 78° C. the gas would be precipitated as a white snow and a polar cap would form; during the summer, as the temperature rose above this point, the snow would be converted into gas and the polar cap would gradually dissipate. Being heavy, the gas would remain close to the surface and would move toward the equatorial regions, following the lowest portions of the surface. This is exactly the course that water pursues and all the phenomena, except vegetation—and that is an inference from the water hypothesis, not an argument for it—would be as well explained by the presence of carbon dioxide as by water.

One of the strongest arguments against the presence of aqueous vapor is embodied in a recent paper by Dr. Stoney* and to get its full bearing we must consider it somewhat in detail. It is well known that if a body be fired up into the air, gravity will continually diminish its upward velocity and will eventually bring it back to the ground. The greater the velocity, the higher will the projectile ascend and the longer will it take gravity to reverse its motion. If we keep increasing the velocity, a limit will finally be reached beyond which gravity will not be able to reverse the motion, and the body will move off in an orbit of its own. Now, according to the kinetic theory of gases, all the molecules of a gas are in rapid motion, some with less, others with greater velocities. Constant collisions decrease the velocities of some and increase the velocities of other molecules; the mean velocity—using this term for the velocity whose square is the mean of the squares of the individual molecular velocities—is, however, constant for a given temperature and according to Clausius is expressed by the relation

$$w = 111.4\sqrt{\frac{T}{\rho}}$$

in which $T$ is the absolute temperature and $\rho$ the density of the gas, that of hydrogen being taken as unity. Using an absolute temperature of 207 (— 66° C. the assumed temperature of the

* Of Atmospheres upon Planets and Satellites by G. Johnstone Stoney. Scientific Transactions of Royal Dublin Society, Vol. VI, No. 13. Reprinted in *Astrophysical Journal*, January, 1898. A popular account is given by Sir Robert Ball in an article on Mars, reprinted in Pub. of Ast. Soc. of Pac., Vol. V, No. 28.

upper air when hottest) this equation gives 1605 meters per second as the mean velocity of hydrogen, 1133 meters as that of helium and 534 meters as that of aqueous vapor. Now if enough molecules attain a sufficiently great velocity, they will move off through space and thus the gas will gradually escape from the atmosphere; "and the evidence goes to show that a velocity which is between nine and ten times the velocity of mean square, a velocity which is able to carry molecules of either hydrogen or helium away from the Earth, is sufficiently often attained to make the escape of gas effectual."

Assuming for our atmosphere a height of 200 kilometers, it is easy to calculate the least projectile velocity which would enable a body to escape from the Earth. This is 10.5 kilometers per second, a velocity over six times the mean velocity of hydrogen at the assumed temperature and nine times that of helium. Inasmuch as free helium is not present in the atmosphere, we conclude that a velocity nine times that of the mean is often enough attained to enable helium to escape. In the same way we find that a velocity, twenty times the mean velocity, must be attained to enable water to escape; this loss of water has not occurred in sensible quantities; hence it is legitimate to infer "that in a gas a molecular speed of 9.27 times the velocity o mean square, is reached sufficiently often to have a marked effect upon the progress of events in nature; while on the other hand a molecular speed of 20 times the velocity of mean square is an event that occurs so seldom that it exercises no appreciable influence over the cosmical phenomena which we have been considering."

The application of the results to the planets indicate their general accuracy. For the Moon, a projectile velocity of 2.38 kilometers per second is sufficient to enable a particle to escape; consequently a gas 39 times as heavy as hydrogen would escape from the Moon with the same facility that helium does from the Earth. Oxygen, nitrogen and aqueous vapor would escape with greater facility than hydrogen does from the Earth, and argon and carbon dioxide with greater facility than helium from the Earth. This agrees with, and explains the fact that no atmosphere has been detected on the Moon.

From the known dimensions of Mars, we find that a projectile velocity of 4803 meters per second is sufficient for the escape of a body. From this we find 9.57 as the density of a gas which would escape as freely as helium does from the Earth. As Mars

receives less heat from the Sun than the Earth, it is probable that the highest temperature in the upper layers of its atmosphere will be lower than that on the Earth; on the other hand, it is probable that heavier molecules than helium escape from the Earth. "Taking both considerations into account, it is legitimate to infer that water, in which $\rho = 9$, cannot remain on Mars." As the density of carbon dioxide is over twice this value, it is probable that this gas exists on Mars.

In view of the temperature difficulty and the researches of Dr. Stoney, it is probable that we will have to revise our conception of the surface conditions on Mars.

CINCINNATI, March 24, 1899.

## THE SUN DIAL OF AHAZ.

BURTON SCOTT EASTON.

FOR POPULAR ASTRONOMY.

I should like to add a small contribution to the somewhat extensive mass of commentary on Dr. Morrison's now apparently celebrated article in December POPULAR ASTRONOMY. I shall differ from others, however, in considering only the mathematics of the question.

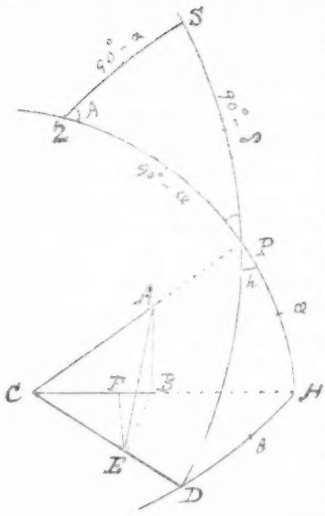
On page 543 of the December number equation (3) is:

$$\tan \theta = \sin (\varphi - \psi) \tan h$$

where $\varphi$ is the astronomical latitude of the dial, $\psi$ the inclination of its plane to that of the horizon, $h$ the hour angle of the Sun reckoned from upper culmination, and $\theta$ the angle between the shadow of the stylus and the line drawn to the north point, *i. e.*, the "reading" of the dial. When the shadow moves forward $\theta$ increases for the Sun to the west of the meridian in general, and if the shadow retrogress $\theta$ must *decrease* for the Sun to the west. Or what is the same thing: *If the shadow retrogresses $\theta$ must have a maximum value.*

Consider (3). Determine $\varphi$ and $\psi$ in any way whatever. That done, sin $(\varphi - \psi)$ is a constant. Then, if $\theta$ has a maximum value, so does $h$. But that is impossible since the hour angle of the Sun simply increases forever with uniform regularity. Then $\theta$ cannot have a maximum. *And the shadow can never retrogress*, no matter what the values of $\varphi$ or $\psi$ are. (For the stylus directed towards the Pole.)

What then has Dr. Morrison done? From his equations (4)




and (5) he finds that when the angle *ZSP* in his figure (the parallactic angle) is ninety degrees, the angle *A* has a maximum value. Now *A* is the azimuth of the Sun,—the angle included at the zenith between the meridian and the great circle passing through the Sun. (As Dr. Morrison says, it is immaterial whether *Z* be the true zenith or that of the inclined dial plate). Then equation (5) gives the hour angle of the Sun, at which its azimuth is a maximum. But this is not a maximum value of the hour angle. And when the azimuth of the Sun is this maximum on either side of the meridian, equation (8) will give the corresponding value of the angle $\theta$ between the shadow of the style and the line to the north point. But this is not a maximum value of $\theta$,—the shadow cannot reverse the direction of motion at this point. Similar considerations apply to (9).

The value of $\chi$ is not a factor in the discussion. The effect of its presence is to move the dial plate zenith towards the south, and so to grant the possibility of the Sun's azimuth having a maximum value for dials without the torrid zone. But it will have no other effect.

The error is still further exemplified by the explanation which follows for the benefit of the non-mathematical reader. On page 548, line 12, he is told to consider the case of "a *vertical* pillar or style." Which is an entirely different thing and there the shadow angle *is* a direct function of the azimuth.

I notice that on page 103 of the February number Dr. Morrison speaks of this point as having been called to his attention by Mr. Saunders, but dismisses it by simply saying that the "equation (2)"—or (3) which is the same thing—"does not admit of the treatment" for determining a maximum. It does *not*, and that is just the difficulty.

The discussion has been rather amusing in some respects as, *e. g.*, the naive remark of one gentleman that a certain feat would give an omnipotent Deity "too much trouble" (!) but it is none the less out of place in an astronomical magazine.

UNIVERSITY OF IOWA, Iowa City, Ia., March 23, 1899.

## A HINDU ASTRONOMER.

W. E. PLUMMER.

For Popular Astronomy.

We have had presented to us from time to time more or less trustworthy accounts of the position and extent of astronomical science among the Hindoo race. Such works have generally been unsatisfactory. Some have been written by men who have studied with care the language in which the records have been written, but whose knowledge of physical science has been inadequate. Others have been produced by physicists, more or less ignorant of the language, but based on the preliminary studies of philological writers. The one class, unaccustomed to modern accuracy, has perhaps been inclined to exaggerate the progress made, the other looking for a precision which might be rendered useful in modern researches have treated the results with some impatience. Neither class possibly could fully sympathize with the prejudices, appreciate the chilling and depressive influence of tradition, or recognize the laborious methods necessarily practiced. But in these last days, attention has been called to a work on practical astronomy, written by one intimately acquainted with the methods of his predecessors and trained in their knowledge and science. He is practically in the same position as a Hindu student some centuries since. We are taken back to the middle ages before telescopes began to be, and before accumulated knowledge and experience was at our service. And not only have we in this book, which those who can read Sanscrit may study with profit, the author's own theory and results, but we have an introduction by one, who, while possessing a respectful veneration for the practices of his forefathers has enjoyed the advantage of a modern education in physics. We have here an Eastern and instructed patriot endeavoring to make the student of the West aware of what his countrymen have accomplished by the means they have to their hand, the reverse case to that to which we have been accustomed, in which the Western student strives to enter into the thoughts and intentions of the East. For Professor Chandra Ráy, M. A., the editor of the book, holds the chair of Physical Science at Cuttack, and though he modestly disclaims any special and intimate knowledge of astronomy, those who read his interesting preface will perceive that he is more than competent for the task he has undertaken.

The main object of the Professor is to introduce us to his countryman, Chandrasékhara, the author of the Siddháuta Dar-

pana, to make us understand his character and his aspirations and to ask for a respectful consideration of such untiring work as Chandrasékhara has exhibited. In so doing Professor Ráy draws a picture as pathetic as it is instructive. It seems impossible to imagine a man beset with greater difficulties than Chandrasékhara and rising superior to them all. Born of an aristocratic family, belonging to the government race; with every inducement and example to lead a life of oriental ease and luxury, he has preferred to accept a position which is regarded with displeasure by his equals and contempt by his inferiors, if by so doing he can advance his favorite science, which might be more accurately described as his religion. Suffering acutely from ill health, he will attempt no alleviation that is not sanctioned by traditional authority, and this Hindu of the Hindus laboriously piles up his observations in the intervals his paroxysms leave free. These may be regarded as objective drawbacks. Subjectively, he is hampered by his devotion to authoritative teaching. The only instructors he can tolerate are the old writings of Hindu astronomers: what they contain is not only science, but whim revelation. To correct the tables which they left is a holy task for this severe ascetic, but his work must be accomplished by tools sanctioned by ancient use and authority. No contamination must be permitted through the introduction of foreign matter or more modern methods. For more than forty years he has struggled on in this thankless mistaken road, to shun delights and live laborious days, sustained only by his enthusiasm and perhaps some little recognition by the cultured few. Such untiring energy, such indomitable patience constitutes the pathos of the situation: the instructive side consists in studying whether a man's own efforts and inquiries will lead him, how much one man can teach himself, what advance one devoted life is capable of effecting.

Born at Khandapara, among the hills and jungles of Western Orissa, it need scarcely be said that his only possible Observatory was the blue vault of heaven. His instruments were for the most part the work of his own hands, and the use of a lens or the opportunity of grinding one, does not seem to have penetrated to that remote outskirt of civilization. The principle of the pendulum seems also to be unknown and the clepsydra has to do duty for a sidereal clock. He possessed indeed an armillary sphere but the instrument on which he relied for the purposes of observation was a mánayantra, a kind of tangent staff. It consists of a thin rod of wood, twenty-four digits long, at one end of which is fixed another rod at right angles in the form

of a tee square. The cross piece is notched and also pierced with holes at distances equal to the tangent of the angle formed at the free extremity of the other rod. It seems scarcely possible that such a rude instrument would enable him to make observations nearer than a degree. He was seen to measure with ease, and presumably a fair amount of accuracy the distance between Mars and Venus, at the time about six degrees apart. The instrument was made upon the spot on this occasion but we are not told how he ensured the rectangularity of the cross piece, or determined the length of the radius. His observations when made are recorded on the palm leaf that served his ancestors for a similar purpose years and years ago.

Could tools such as these enable any one, unswayed by dogmatic teaching and free to follow his own judgment, to determine the general principles of planetary motion. Professor Ráy compares the author and, as we think, justly, to Tycho Brahe. There is no attempt to draw a parallel between him and Kepler for Chandrasćkhara is essentially an observer. Theory and explanation do not come within his plan. There is no suspicion that he ever appreciated the idea of relative motion. Necessarily, therefore, there is no attempt to suggest a law of force. Mars does not concern him, motion is everything. His observations are made with the main purpose of predicting the places of the planets. And now it must be admitted with regret, but without surprise, that Chandrásekhara, like his predecessors, imagines a stationary Earth, about which the Sun and planets revolve. Notwithstanding this heresy, I still venture to plead for interest in, and respectful sympathy for, this lonely astronomer. He has simply never experienced the necessity of a different hypothesis. We may talk glibly of a theory of gravitation, but at the same time our only reason for accepting it is that it explains observed facts. And our author is in precisely the same position. He can explain observed facts and as we shall see presently compute ephemerides that represent the places of the planets within the errors of his observations. We can do no more. The limit of the accuracy of prediction must be settled by the accuracy of observation. Of what use is it to tell Chandrasćkhara that if he made more accurate observations his rough ephemerides would fail to represent the places of the planets. He can apply the same argument to us, who adopt the Newtonian philosophy. He may urge with perfect justice that if we made more correct measurements, we should find the inadequacy of our theories. Indeed he might say with some reason "Have not some of your most learned and trusted astronomers suggested

that your famous law of attraction is not exactly as the inverse second power of the distance, as Newton taught, and which you quote as a shibboleth? Have you not found it necessary in some of your researches to make a small change in the law of central force?" You may be loth to accept the suggestion but the fact remains that some emendation has been forced upon you. A paradoxist, who brandishes in our faces some crude notion that he thinks he has discovered is a most objectionable person, but I hope no one will do the patient Chandrasékhara the injustice to place him in this unworthy category. He renders us a distinct service by compelling us to review the grounds of our belief. Some violent awakening is at times necessary to make us appreciate the necessity for examination, and we have no hesitation in saying that our Hindu author can give better reason for the faith that is in him than many of us who parrot-like quote a formula, we can neither prove nor understand. There is a tendency on the part of some to regard any long established theory as incontrovertible, to surround it with a halo of respectability, which it is bad form to disturb, and even to forget the arguments by which it was originally established. Moreover there is a pernicious habit, we see it illustrated in some so-called textbooks, to support a recognized theory by childish illustrations and arguments which come to be looked on as proofs, forsooth, and which are accepted by those who rely upon authority, leaving unconvinced the man who struggles hard to reconcile the new teaching with the results of his own observations. Chandrasékhara seems to have tasted some of this spoon-food and to have rejected it with the contempt it deserved. Professor Chandra Ráy mentions that a miserable Uriá translation of a Bengali compilation from English popular astronomy fell into his hands, with the result that a careful study merely conformed his idea of a stationary Earth.

The theory which Chandrasékhara holds as sufficient approaches, so far as we have understood it, closely to that which Tycho maintained. One is tempted to ask whether it is necessary to pass through the fallacies and misconceptions which disfigured Tycho's theory, in order to arrive at the simpler and satisfactory conclusions of the Newtonian principles. The centers of the orbits of the planets are, in both the Tycho and the Hindu philosophy, placed in the center of the Sun and the Sun turns round the Earth carrying the orbits of the planets with it. The Moon is made a satellite of the Earth in all theories. Elliptic motion does not appear to have much attraction for the Hindu philosopher, though in his solar theory he has succeeded in determining

the greatest equation to the center with considerable accuracy. Determinations of distance must necessarily present difficulties. A single observer on a supposed stationary Earth has no means of measuring a parallax. In this difficult position the problem seems to have been solved by the ancient Hindus in a very peremptory, if very inaccurate manner. This rule was to assign to each planet a parallax equal to $\frac{1}{15}$ of its mean motion. This gives approximately four minutes to the Sun, and fifty-three to the Moon, quantities which permit the determination of the phenomena of an eclipse with not too great an error for Hindu almanacs but troublesome to a man of Chandrasékhara's accuracy. In his reading however of the ancient astronomers he found that the Sun's diameter had been misquoted, and by correcting the error he was led to adopt 22″ for the Sun's parallax. He used for the Moon 56′ 28″. This gave him for $(\pi - p)$ 56′ 6″, while the European value is 56′ 40″, quite a sufficient approximation.

Not only in constructive science does Chandrasékhara resemble Tycho, but it is not a little interesting and remarkable to notice, that he bases his reasons for rejecting the Newtonian idea on the same grounds that Tycho found sufficient. Authority in both cases plays an important part in warping the judgement. Tycho as a good churchman, would enunciate no doctrine that would bring him in conflict with the church, and the Hindoo will accept no proposition that is opposed to Sastric authority. Both raise the same scientific objection, that if the Earth moved around the Sun, every star should describe a curve in the heavens of the same apparent dimensions as the Earth's orbit seen from the star. The absence of an annual parallax disturbed both philosophers. The quest of this critical quantity which has been so fructiferous of discoveries in Europe does not seem to have penetrated to the far East. It may come later, but the time of figs is not yet.

Nevertheless, industry and perseverance have taught the Hindoo much. In considering what he has accomplished, I suggest that we may pardon his unwillingness to accept as proved those facts that have not been brought within his capacity for observation. To test his accuracy as an observer, we will take first of all the values he has assigned to the periods of the planets. In such an enquiry it is necessary, or at least convenient, to compare the positions of the planets at two distinct epochs. But the earlier epoch was practically wanting to Chandrasékhara. He is restricted to the use of Hindu observations and from the days of Bhaskara, about the middle of the twelfth century, no

human astronomer has possessed the necessary competency or perseverance to collect the necessary data. Bhaskara has left no record of his observations, but he has constructed tables, which it must be supposed represented observed facts in his time. Given his elements and his date therefore it is possible to determine where the planets were situated at his epoch. There may have been difficulties connected with chronological reckoning and the exact determination of the number of days that have elapsed since Bhaskara worked and his countrymen worthily followed in his footsteps. Over this preliminary difficulty our Hindu astronomer has triumphed satisfactorily. The period of the Sun and Moon has been assumed without alteration from the Seddhánta Surya, but from the table annexed will be seen the approximation that obtains in the case of other members of the solar system.

SIDEREAL PERIODS IN MEAN SOLAR DAYS.

| | European Value. | Siddhánta Seromain (1150). | Siddhánta Darpana (1850). |
|---|---|---|---|
| Sun | 365 25637 | 365 25843 | 365.25875 |
| Moon | 27.32166 | 27 32114 | 27.32167 |
| Mars | 686.9794 | 686 9979 | 686.9857 |
| Mercury | 87 9692 | 87.9699 | 87 9701 |
| Jupiter | 4332.5848 | 4332 2408 | 4332.6278 |
| Venus | 224.7007 | 224 9679 | 224.7023 |
| Saturn | 10759.2197 | 10765.8152 | 10759.7605 |

Having regard to the comparatively slow motion of Jupiter and Saturn, and the nature of the instrument used, it would have been a surprise if a closer approximation to their true periods had been made.

Again, consider the inclinations of the orbit to the ecliptic. This is a problem which at no time is simple, and is not made any simpler by confining the attention to geocentric observations, yet by processes, which if not fully understood can yet be appreciated, the result of the Hindu astronomy is made to approach in accuracy modern results. We give the comparison with the western researches and add the mediæval Siromani values to show the stride in advance that has recently been effected by the perseverance of Chandrasékhara:

VALUES OF THE PLANETARY INCLINATIONS.

| | Modern Value. | Siromani. | Darpana. |
|---|---|---|---|
| | ° ′ ″ | ° ′ | ° ′ |
| Mercury | 7 0 8 | 6 55 | 7 2 |
| Venus | 3 23 35 | 3 6 | 3 23 |
| Mars | 1 51 2 | 1 50 | 1 51 |
| Jupiter | 1 18 41 | 1 16 | 1 18 |
| Saturn | 2 29 40 | 2 40 | 2 29 |
| Moon | 5 8 48 | 4 30 | 5 9 |
| Obliquity | 23 27 | 24 0 | 23 30 |

In the Solar theory, Chandrasékhara has determined and used the greatest equation to the center with surprising accuracy. It is not a little puzzling to understand how in his primitive observations, he was able to subdivide the minute into seconds. But for this constant his commentator gives us the value of 1° 55′ 33″ compared with 1° 55′ 19″ of modern astronomy. It is, however, in the lunar theory that the greatest triumphs have been won. The three largest inequalities in the lunar theory, the evection, the variation and the annual equation, two of which were clearly recognized by Tycho and one at an even earlier date do not appear to have been considered in the early Hindu astronomy. At least, Bharkara makes no mention of the largest of those which was first noticed in the tenth century. It is believed that Chandrasékhara had no means of knowing of their existence from any foreign source, but succeeded in detecting all three from his own observations and assigning to them very approximate values. Modern astronomy gives the following values against which we place the amount assigned by the Hindu:

| | ° | ′ | ″ | | ° | ′ | ″ |
|---|---|---|---|---|---|---|---|
| Evection | 1 | 20 | | | 2 | 40 | |
| Variation | 0 | 39 | 31 | | 0 | 38 | 12 |
| Annual Equation | 0 | 11 | 9 | | 0 | 12 | 0 |

It will be noticed that the value assigned to the evection is twice that now approved, but this is to some extent corrected in the theory by reducing the maximum value of the equation of the center. We are accustomed to consider this 6° 18′ and with the maximum evection to make the total 7° 38′. Chandrasékhara gives 5° 1′ for the equation in the center and with 2° 40′ for the evection makes a total of 7° 41′. It would be very interesting to see how far the values assigned and the methods employed could be assimilated to modern results, bnt unfortunately the rules for determining the position of the Moon are not very clearly given. This is of the less consequence, however, since Professor Ráy has given the computed places from Hindu data for particular dates, of which we select one. There will always be a difficulty in making the comparison owing to the motion of the equinox and the value assigned to the precesion. It is particularly complicated in this case owing to the error in the length of the sidereal year, and the motion of the Sun in this time, about 8″. Professor Ráy has gone very carefully into the question, both on account of its importance as a constant of astronomy, and the possibility of determining the date of the origen of astronomical enquiry in Hindustan. To follow him in

detail through this section would carry us too far from the subject of this sketch. We will merely say that the value of the precession he has deduced from Chandrasékhara's work is 57″.615, of which rather more than 8″ is due to the motion of the Sun in the difference of time between the Hindu and the European year. But much of this difficulty will be removed, if we consider the differences of longitude between the planets, rather than the absolute longitude reckoned from the vernal equinox. The date we select for exhibiting the degree of success that has attended Hindu research is 1894, Jan. 31, 6:00 P. M. In the last column is given the longitudes of the various planets from a Bengali almanac, which it will be seen are far less accurate than the values of the Siddhauta Darpana.

| | Nautical Almanac. Vernal Eq. | | | Siddhauta Darpana Vernal Eq. | | | Bengali Almanac. Vernal Eq. | | |
|---|---|---|---|---|---|---|---|---|---|
| | s | ° | ′ | s | ° | ′ | s | ° | ′ |
| ☉ | 10 | 11 | 54 | 10 | 11 | 56 | 10 | 10 | 34 |
| ☽ | 4 | 4 | 26 | 4 | 4 | 41 | 4 | 2 | 55 |
| ☿ | 0 | 22 | 48 | 0 | 22 | 23 | 0 | 20 | 37 |
| ♀ | 10 | 0 | 33 | 10 | 0 | 58 | 10 | 3 | 10 |
| ♂ | 9 | 19 | 19 | 9 | 19 | 12 | 9 | 18 | 40 |
| ♃ | 0 | 19 | 27 | 0 | 19 | 8 | 0 | 18 | 0 |
| ♄ | 6 | 12 | 43 | 6 | 13 | 3 | 6 | 13 | 8 |

The Bengali almanac, which is said to have been computed from the Súrya Siddháuta may have as much as 4° error in the differences of longitude between the two planets, but the Durpana rarely exceeds half a degree in error.

We consider this the most practical result which the unassisted astronomer of Orissa has achieved. The ritual of the Hindus demands a constant attention to the position of the heavenly bodies, and therefore an almanac is a necessity in every household. Professor Ray tells us that no two of the almanacs agree in their facts and that considerable confusion exists. It would seem to the Western mind a very easy matter to introduce the Nautical almanac and establish uniformity by its use. But this we are told is impossible. Whatever improvement is to be effected, whatever alteration is to be made, must originate in the religious community itself, and by the accuracy this Hindu astronomer has introduced, he has rendered a great service to his co-religionists, whose beneficial effects have not yet made themselves fully felt. Perhaps it was in recognition of this service that the government conferred upon him a title as a mark of encouragement and respect. It is the only reward he has received, except the consciousness of well doing. Professor Ráy begs for him a larger measure of assistance and sympathy, with which appeal we

would respectfully associate ourselves. 'Geniuses,' says the sympathetic editor, 'are like delicate plants, never plentiful anywhere, and depend on tender care for growth, development and fertility. Let us, therefore, hope that the past neglect of his countrymen may yet be compensated and that better days may yet dawn upon our old and crippled observer of heavenly bodies.'

LIVERPOOL OBSERVATORY, England.

## THE FUNCTION OF CRITICISM IN THE ADVANCEMENT OF SCIENCE.*

FRANK HAGAR BIGELOW.

The history of the development of human knowledge towards its goal, perfection, which may also be regarded as the history of science itself, shows that there has been a gradual improvement in the number and precision of the facts known to man, in the mechanical and intellectual skill with which these are handled, and in the forecasting of the ultimate truths to which they point. This advancement in scientific knowledge has been compared to the gradual rise in the level of the sea upon the shore, which is marked by the ceaseless beat of the waves upon the rocks and sands, with the advance and the retreat of individual billows, and the bewildering swirl and foam of breakers and the returning undertow. We may not inappropriately compare the practical aspects of criticism to the waves of research which advance and retreat, in dependence upon the mighty ocean that impels it onwards, sometimes concealing with strife and controversy the grand harmony as it seeks its lawful expression.

Now, while it may be taken for granted that all educated persons, especially those who have attempted to add something to the sum of human knowledge, have general ideas about criticism and what it attempts to do, yet I may be permitted to doubt that every one here has clearly thought out what the organic nature of the function is, and in what ways it operates to the best advantage. At least it may not be inappropriate to devote the time at my disposal to making some attempt to bring out the leading features of a subject so vital to the interests of science, with the hope of stimulating students to a more correct employment of this agency than has been uniformly the case in the

* The annual presidential address delivered Jan. 7, 1899.

past. It is proposed, therefore, after a few remarks intended to illustrate the misuse of criticism, to proceed with the statement of the canons of criticism and the recital of a few examples which illustrate them in the branches of science with which I happen to be partially acquainted. A complete exposition of my theme would involve the rehearsal of the entire range of scientific discovery since man entered upon the process of finding out things carefully and systematically.

The idea of criticism is apt to excite the thought of something very disagreeable, simply because no one enjoys the application of corrective measures to his lack of information, to his pride of reputation, and generally to his self-esteem or self-will. Ignorance brought home publicly and presumption exposed to rebuke are terrible weapons to punish those who break into Nature's great preserve by any other than the narrow way of labor that alone leads to ultimate truth; yet the genuine scientist cannot but rejoice to see his errors exposed, for few determined searchers in Nature's treasure-house have any satisfaction in following a barren vein or a phantom promise. Hence we may justly conclude that every true student is his own best and sharpest critic, so far as in him lies. Criticism is not something to be avoided or dreaded, if it is sound. It is rather one of those labor-saving devices that has fortunately been bestowed upon us to keep us from too much useless work.

On the other hand, what can be said that is too severe of the trash, which is falsely called criticism, that encumbers nearly every journal devoted to scientific information. There are two classes of critics to whom no quarter should be shown, namely, the "omniscient critic" and the "professional critic," the man who knows all about all subjects, and the man who knowing little, yet criticises all things in heaven and Earth. The former, that is the all-knowing critic, may really know some subject very well, but has conceived that this is guaranty that he may express equally positive views in many other fields of learning; the latter, the regular critic of all subjects, is usually equipped with sarcasm, innuendo, and a large phraseology. How often it has happened that men of power in one field of work have not modestly confined their criticism to that particular department of work. How common a thing it is for a man who has a reputation in some direction to become a sort of general authority and have all sorts of essays, papers, and treatises referred to him for his opinion. Now we all know very well that this sort of thing is practically impossible in the advancing speci-

alization of science. The ramifications are too minute and too subtle for any one man even to keep posted on the bare facts in many directions. Hence this class of critics must necessarily go. The journals and magazines requiring the monthly quota of review material have been largely at fault in sustaining this class of criticism, but they ought to adopt the one obvious method of meeting the difficulty, namely, by sending for review the new paper to the best authority known, and then accept for publication his comments over his own signature. Let us do away with this anonymous kind of criticism. The best man writes sympathetically, he expresses valuable opinions, and generally courtesy and gentleness will mark his work. That is the kind of review which authors welcome, and it does not exasperate, nor does it discourage further endeavor. The harsh and destructive criticism is as seldom needed as a whip for a willing child; for surely, after a man has for years sought earnestly and painfully to secure a more correct knowledge of his subject, can anything be more distressing and unjust than to see his work caricaturized by a critic who plainly does not understand it—by one, for instance, who selects the wrong case out of two or three which may occur, and proceeds to destroy that as if it really were the author's conception.

Then there is the other kind of critic just mentioned, the man who is really ignorant and yet makes his copy by writing something in general terms, or else in an entirely incorrect manner, about another man's production. Such men are not mathematicians, they are not philosophers; they may be general readers, but they do not possess the critical faculty of seeing things exactly as they are. We might spend our time in enlarging upon these faulty styles of criticism, and they would make very interesting reading, though perhaps too personal for comfort, but we hasten onwards to some exposition of that just, genuine and noble criticism whose application to the advancement of science it is a pleasure to facilitate in every possible way.

We may arrive at a definition of the logical process of criticism, philosophically considered, by referring to the principle of identity—A is A, or A is not B. This is the ground of the intellectual process of judgment. Having prepared two or more concepts, through sense-perception or intuition, the mind advances by comparison, and the idea of identity or non-identity, to a judgment or affirmative proposition; out of a group of such identity judgments we possess the faculty to generalize these primary elements into laws and universal ideas, which are the

final products of human intelligence. Concept, judgment, general law—these are the three stages in the gradual construction of knowledge. Now, criticism is essentially the exercise of the faculty of knowing in all these three stages, but especially it is that of judgment, which holds the central transitional place. We must know that the objects of knowledge are true and not false; we must group these objects by similarities and identities; we must perceive the features common to these groups in order to construct laws and theorems. Theories are the tentative and probable forecasts of the laws which may embrace many groups of identity judgments, and it is the dream of the human mind to rise at last to the central unifying law, the attainment of which is the goal of philosophy. The comprehensive unit idea has not yet, except very vaguely, floated before the most exalted human mind. Science in its present stage is chiefly concerned with building up correct concepts, commonly called facts; progress has been made in the province of identity; and some working laws, holding good in limited realms of thought, have apparently been attained, but yet we can scarcely claim to be much better off than blindly groping after final truth.

Practical ideas about criticism can easily be drawn from this general analysis. Suppose some new fact or law is proposed, then the first test to be applied is whether it is *true* or *false*; the second is whether it is *new* or *old*; the third is its *place* in the general scheme of knowledge. If what is offered in evidence by an author can be shown to be *inaccurate, incorrect, or false* by its lack of harmony with other facts or pieces of evidence, by some internal inconsistency, or by its incompleteness, then it fails, and by just criticism it is thrown out. But, having passed this stage successfully, it is next necessary to examine its bearing upon other previously admitted parts of knowledge to discover whether it is *new* or is in reality *old*, being merely something presented in a different aspect, disguised perhaps unintentionally and unconsciously, but yet being merely a phase of some other product of experience. Finally, having been passed upon by criticism and believed to be *true* and *new*, then it is necessary to determine its dimensions, so to speak, its size, whether it is smaller than some more comprehensive law and is to be placed as a subordinate part of it, or whether it is larger and contains other laws within itself—that is to say, we must determine its rank in the hierarchy of ideas. These three canons of criticism, *truth*, *newness*, *rank*, corresponding as they do to the three forms of thought, *concepts*, *judgments*, *general laws*, respec-

tively, are thus easily comprehended and should be consciously practiced by scientific critics; for we claim that criticism should become as much a function of science as the discovery of facts and laws, and indeed it is here contended that this is the great intellectual process by which advancement can be profitably assured. The lack of system in criticism, the haphazard controversy, involving bitter feelings, slander, and errors of statement on the part of the critic, far less excusable than the honest but mistaken results of laborious investigation, should never have any place in science, properly so called. These questions should be put to himself by every searcher after truth and by every critic of other men's work:

1. Is the concept and the fact *true*?
2. Is it something *new*, or is it already known in some other aspect?
3. Is it *subordinate* or *superior* to the allied facts and laws with which it is compared?

The first thought which comes to mind regarding the practical operation of such critical canons is this, that with the growth of human knowledge it is becoming harder every year to be a sound critic in any department of knowledge because of the immense range and the tremendous catalogue of facts which one must acquire in order to be able to apply the first and the second canons of *truth* and of *newness* to any candidate for matriculation in the school of science. The extension is fast becoming so great that our individual specialization is beginning to make us quite incompetent critics, except in very narrow fields. Furthermore, many new ideas have their range legitimately in several adjacent departments, as, for instance, astronomy, geodesy, meteorology, terrestrial magnetism, and pure physics, so that the comprehensive minds who can conquer definitely these vast ranges of knowledge and are equipped for intelligible criticism must diminish in number. That is why the pseudo-critic already alluded to does so much harm by reason of his own lack of preparation in the premises. On the other hand, we may gain hope from the fact that the application of the third canon, of *rank*, is gradually building up sweeping incisive principles and laws so that a multitude of facts are already securely subordinated in their proper order. What a splendid power of hierarchy pertains to the Newtonian law of gravitation, to the mechanical equivalent of heat, to entropy, to potential and kinetic energy, to the principle of conservation of work, energy, to chemical equivalents, to the electric and magnetic cross-con-

nections. If the extension of the elements of knowledge threatens to overwhelm our powers of endurance, we may take refuge in the belief that the profoundly mysterious power of the mind, by which unification in laws of deeper intensity is going on *pari passu*, will be ultimately able to keep the scales of judgment and criticism evenly poised in a just equilibrium.

It is evident that the memory of the multitude of incidents which have occurred tempts one to relate many stories of faulty criticism, and thus to draw aside from the real object of this address, namely, to give a few examples of the sound criticism which has materially contributed to the advancement of science. However, before passing on to that part of the subject, one may be permitted to illustrate the erroneous use of the three principles which have been laid down for the guidance of critical efforts. Nothing is more common than for a critic to be wrong about his own comprehension of the facts in the case, and this may be partly due to a lack of complete information or to a tendency to jump to conclusions without sufficient preliminary study.

1. A traveler on the western arid plains found some hard balls lying on the ground about the size of a goose egg and coated with a white shell which contained a mass of tough, grass, hair and other loose material. Specimens were duly sent east for expert opinion as to their nature, with an account of the conditions under which they were discovered. The answer was returned that these balls were buffalo cuds which had become hardened by exposure. Nevertheless the same traveler afterwards saw some bugs—tumble bugs he called them—who industriously formed little balls on the ground and then rolled them along till they had grown beyond their strength to move, having by that time acquired considerable size when they were abandoned. So the balls never performed in the function of buffalo digestion.

2. A gentleman had a favorite tree in his yard which suddenly began to turn yellow at the very top, the trouble spreading rapidly downward and involving the entire foliage. An expert was called in to assign a cause for this disaster and suggested that the leaves had been killed by spraying too much with oil or tobacco. But the owner was able to state that he never had any apparatus for this purpose in his possession, and that certainly no poisonous material had been used on the tree in question.

These are instances of mistaken identity as to the primary facts and show violation of that canon of truth.

3. A pathetic case came into my own experience some years ago. A man sent for examination one of those curious results which are so plentifully derived from the loose reasoning of "planetary meteorology." It was contained on a diagram several yards in length, giving curves, orbits, conjunctions of all sorts of phenomena, and it was quite evident that much time had been expended in its preparation. It seemed to me proper to return some courteous comments in very general language and, as I supposed, entirely concealing my real opinion. Soon a letter arrived from the author thanking me for my favor and asking permission to use my name as an endorsement in the publication which was proposed. He stated that he was an invalid, had been practically confined to his bed for twenty years, and that my words contained the first encouragement that he had received in all this time. One could not but pity the man, considering what it must have meant to him, but I declined to lend my name. The temptation was too strong, and it appeared all the same in his printed paper.

These are illustrations of criticism in which the facts are questioned as to their identity or truth. There is a class of erroneous criticism which comes from comparing two things not identical in their nature. Thus the comparison should be, *A* is *B*, but it was stated that *A* is *C*, whereas in reality *B* is not *C*. This occurs when, for instance, there is more than one possible case affording an explanation of the phenomenon in question, and the application is made to the wrong case: (1) Several years ago an attempt was made to explain the peculiar curvature of the rifts of the solar corona which are photographed during an eclipse by comparing them with the lines of force surrounding a spherical magnet with uniform internal magnetism. A critic drew out his equations and the corresponding lines of force and claimed that they did not agree as was supposed. A brief examination of the formula revealed the fact that he had taken the companion case of a spherical magnet immersed in an independent external field, which changes the form of the curves near the surface. He simply applied the wrong case. (2) Another somewhat similar instance, but more complex in its details, came also to my notice. Three cases may occur in which the lines of force in a uniform magnetic field are distorted by placing a permeable substance within it, since the lines always seek the path of least resistance. The first is where a spherical solid as an iron sphere is placed in such a field, the lines taking on a system of curves determined by the strength of the field, the permeability and shape

of the solid; secondly, if a spherical shell with a hollow interior is placed in the same field the lines assume similar but really different curves, and a part of them pass across the hollow (Barlow's Problem); if in the third case there is a permeable shell filled up with a substance impenetrable to magnetic lines, then the curves are different from either of the other cases, being more sharply exflected at the poles. It has been my conclusion that the earth, having a permeable shell and being filled with a material nucleus which entirely turns aside the lines of an external magnetic field, is comparable to the third case, and that in this way a number of observed phenomena find their explanation. A critic, however, carefully worked out the consequences of the second case and found that it did not agree with my exhibition of the phenomenon. It was contended that this criticism is inapplicable, because of mistaking the case which ought to be employed.

Mistaken identity is an exceedingly common error which may befall any investigator, no matter how honest and intelligent, when groping in the dark after hidden facts of nature, and it is no discredit to him that the advancement of science discloses this state of affairs. Indeed, so far the history of the progress of science has consisted chiefly in the superceding the views of one generation by those of another, which possesses wider knowledge and deeper experience derived from trial and practice. (1) For nearly a hundred years the problem has been before magneticians regarding the cause of the great disturbances which occasionally sweep over the Earth's magnetic field and stir up currents of electricity in the crust of the Earth, sometimes so strong as to paralyze the operations of the electric telegraph lines. It was always assumed that the disturbances and the ordinary diurnal variations of the needle had the same source. Careful investigation of the subject was made mathematically by reference to certain observations, and it was concluded that the Sun as a magnet could not be depended upon to produce such effects without imposing excessive conditions. The development of two independent external magnetic fields surrounding the Earth, having their seat respectively in two distinct physical conditions, as given by a discussion of the observations, has recently placed the problem in a new light; for it is shown that the disturbances belong to one of these fields, and the diurnal variations to the other, and that thus the early efforts to elucidate the subject were based on identifying two things which are really independent. Such is the criticism that has been recently advanced in this direction, namely,

that the old position of magneticians fails because these two phenomena have different sources, while it was assumed that the same physical condition was behind each of them. (2) I have also ventured to make a criticism of about the same kind, though in connection with a much more difficult subject and one which may properly be held open for further discussion. It has been assigned by a very eminent authority as a reason for excluding the Sun from consideration as an important agent in the disturbance of the Earth's magnetic field, that the work which it would be required to do to accomplish such an effect at the Earth would be entirely impossible. This argument, if true, would cut us off from the only solution of this great problem which appears to be in the least hopeful; for it is shown to be a fact beyond controversy that the envelopes of the Sun and the Earth are continually passing through a series of complicated synchronous variations. Now, something must connect them. What is it? If the proposition is true that the Sun's work must be excessive, and that therefore there is no connecting bond, then the problem is well-nigh hopeless. But we cannot yield to this view without further examination. The result of analysis is that there are three fundamental cases for consideration:

1. The electrostatic case, magnetic energy vanishing.
2. The magnetic case, electrostatic energy vanishing.
3. Joule's heat, electrostatic and magnetic energy.

It takes more work to adjust the variations of the energy due to electric currents when propagated through an electrostatic field than when transmitted in a magnetic field; hence if the Sun has a variable magnetization, due to magnetic masses or electric-current systems passing from one state of equilibrium to another, the static electric field would require much more work than the polar magnetic field for these operations. To charge the electrostatic field from the Sun to the Earth is a tremendous undertaking, but the magnetic field, on the other hand, so reacts upon the source sustaining it as to require a minimum of work. The criticism is that the Sun presents an instance of the second case and not of the first, as has been implied in the mode of thought heretofore presented.

These examples must serve to explain the first canon of criticism on the truth or falseness of the subject matter. They have been introduced on the negative side rather than on the positive to explain some of the principal causes of the failure of critical studies. There are innumerable cases which might have been adduced to show how false facts, false arguments, and imperfect

laws have been thrown out from further consideration by science, and in which work the critics have done good service. There are two points which ought to be mentioned regarding the pseudo-scientist and the pseudo-critic before passing on to the next division of the subject: (1) What ought to be done with those crude scientists commonly called cranks, and their rude productions? The journals, and especially the press, abound with specimens of misapplied information, half-truth propositions, and generally unscholarly productions which may possibly impose upon the less cultivated class of readers. What is the best course to pursue? The temptation is to show them up and expose the false science beneath their words. Many have tried this process, but almost invariably the result has been the increased disgust of the scientific critic, who finds his opponent a good thrower of mud. His effort has resulted in a wider advertisement of the author's feeble ideas, and generally in the belittlement of true science. Undoubtedly the best policy is to let such men and all their works alone. If the ideas are of science, they will live; if not, they will die of themselves, and the world will be richer for the lack of controversy. (2) What should be done when a true scholar is vehemently attacked, his views misinterpreted, and his reputation assailed? Some of our great men have adopted the course of never replying to such criticisms. They refuse absolutely to be drawn into any controversy, and prefer to suffer such injury rather than to be parties to any strife. Indeed, the unhappy heart burnings which have been engendered by scientific wranglings are so notoriously unprofitable, so productive of bitterness and estrangement, that strong men generally prefer to bear patiently these attacks than to be concerned in a story of discord which may be remembered longer than their otherwise good works. What a tale of woe could be gathered together in exemplification of this evil of scientific contention, and how many of them are readily recalled from our recollection of the history of science! There comes a time, however, when even the lion should be roused, and then let us say that he should smite and not spare his false critics. Let the victory be complete.

(TO BE CONTINUED.)

## SPECTROSCOPIC NOTES.

Professor Scheiner, of Potsdam, has succeeded in making an important advance in the study of the spectrum of the Nebula of Andromeda, by showing that it contains solar lines (*Astronomische Nachrichten* No. 3549; *Astrophysical*

*Journal*, March). Such nebulæ as are bright enough to permit an examination of their spectra have long been divided into two classes, those giving bright line spectra, and those giving continuous spectra; the nebulæ with bright line spectra, that is to say, the gaseous nebulæ, including the Nebula of Orion and other large irregular nebulæ, the planetary nebulæ, and the ring nebulæ; and the nebulæ of continuous spectra including the Nebula of Andromeda and the spiral nebulæ. The bright lines of the spectrum of the one class of nebulæ have been the subject of prolonged and important investigation. The continuous spectrum characteristic of the other class has never ceased to be a puzzle and source of speculation. The general belief has been that these nebulæ, unlike the gaseous, are star clusters, and so ultimately resolvable with sufficient power. Professor Vogel, examining the remarkable resolvable cluster in Hercules, Messier 13, succeeded in recognizing dark bands in its spectrum, without being able to determine their character or position. Dark bands have occasionally been reported as visible in the spectrum of the Nebula of Andromeda, but generally the observations have been discredited. With an instrument exceptionally well adapted to the purpose Professor Scheiner attempted to photograph the spectrum of the Nebula of Andromeda, and with an exposure of three and a half hours succeeded in getting a faint impression, in which appeared a strong absorption band, taken to be the *Hγ* line of hydrogen. With an exposure of seven and a half hours the spectrum could be clearly recognized from *F* in the blue to *H* in the extreme violet, with traces far into the ultra violet. A comparison with the solar spectrum taken with the same instrument showed that the two are practically identical, and that the band observed was not *Hγ*, but the stronger adjacent group *G*. The spectrum of the Nebula of Andromeda, and probably of all spiral nebulæ, would thus seem to belong to the same class as the solar spectrum (Secchi, type II; Vogel class II *a*), and the presumption becomes very strong that a spiral nebula is a cluster of stars of the solar type.

---

In the *Astrophysical Journal* for March Professor Dunér gives the results of his observations of the spectra of about 50 stars of class III *b* (Secchi's type IV). Some new facts are brought out, and the observations are largely in agreement with those recently published from the Yerkes Observatory. The observations have been secured under such disadvantages that it is deemed advisable to discontinue them, and to leave further conduct of the investigation to others better equipped and more fortunately situated. The statement that the light-gathering power of the telescope employed (aperture 15 in.) is inadequate, may readily be believed when it is noted that the brightest star under observation is of magnitude 5.5, and most of them much fainter. The observations were further interfered with by the character of the Swedish summer nights, which are stated to be as light as day, and, unfortunately, by the imperfect health of the observer.

---

In the same place Professor Hasselberg describes his observations on the presence of the rare metal Vanadium in meteorites. He finds Vanadium present in minute quantities in stone meteorites, but never in iron meteorites. A foot note states that the only exception to this last is the meteoric iron from Greenland, and expresses the suspicion, already widely entertained, that this is not a meteorite but a terrestrial ore.

---

The address of the President of the Royal Astronomical Society in presenting

the gold medal of the Society to Mr. F. McClean is given in the *Monthly Notices* for February. He refers to Mr. McClean's previous work, the photographs of the spectrum of the sun at high and low altitudes. He describes in some detail the work for which the medal was awarded, the photographic survey of stars down to the $3\frac{1}{2}$ magnitude in both hemispheres, including the examination of the distribution of the various classes with regard to the Milky Way, and the discovery of a remarkable agreement between a certain series of lines in the spectrum of $\beta$ Crucis and a series of lines in the spectrum of oxygen. He adds that it is worthy of remark that Mr. McClean himself took the observations unassisted, and states that the medal was awarded for this research on its merits, and not at all on account of Mr. McClean's gifts to the science of astronomy, of the Isaac Newton studentships at Cambridge and the photographic telescope at the Cape of Good Hope.

In continuance of his recent work on the speculum of the Orion Nebula Professor Keeler contributes to the *Astrophysical Journal* for March a paper entitled 'On some photographs of the Great Nebula in Orion, taken by means of the less refrangible rays of its spectrum.' In ordinary photographs of nebulae it is without doubt the light corresponding to the violet lines in the spectrum that is active in producing the impression on the plate. Professor Keeler's object was to secure an impression as well corresponing to the lines in the green. For such a purpose a reflector is evidently suited, and the Crossley reflector of the Lick Observatory was accordingly used. The photographs taken by the green light were secured by using orthochromatic plates and a suitable color screen to cut out the violet. The exposures of such a plate, and of an ordinary plate, were so timed as to give equal intensity for the region about the trapezium. It was found that when this region was equally intense on the two plates the region of the Nebula about the star Bond 734 was much less intense on the orthochromatic than on the ordinary plate.

In the *Astronomische Nachrichten*, No. 3549, Herr Belopolsky adds $\delta$ Ursæ Majoris to the growing list of stars of variable motion in the line of sight.

In the *Astrophysical Journal* Professor Hastings describes his efforts to increase the degree of color correction of achromatic lenses. A promising combination of two kinds of glass failed because the makers could not furnish one kind in perfect discs. Combinations of three lenses were with great labor devised and constructed. In particular one very promising combination failed because the glass of one member was found to be perishable. Further, combinations of three lenses were found to be impractible because spherical aberration for one color differed from that for another color to a fatal degree. On trying combinations of four lenses Professor Hastings has found one, with the lenses cemented together, giving perfect color correction throughout the whole range of the visual spectrum, from *A* to *K*; and for it the name '*isokumatic*' is proposed. The nature of the curve beyond *K*, which is now hardly negligeable for photographic purposes, is not given. While such a system is important primarily for the spectroscope, it is perhaps not too much to hope that te escopic object glasses of at least moderate size may soon be made on such a plan.

In the same number Professor Deslandres maintains, against Professor Vogel's criticism of his previous statement to that effect, that a small change in

the temperature of the prisms of the spectroscope will effect an appreciable shift in the lines of the spectrum; and that this should be guarded against by those who measure motion of celestial bodies in the line of sight by means of the spectroscope.

*Nature*, March 16, contains an abstract of a paper read at the Royal Society Feb. 23, by Sir Norman Lockyer, in which he describes the temperature range of arc lines and of spark lines of various elements in his scheme of ascending and descending stellar temperatures. He mentions that his observations lead him to confirm Mr. McClean in attributing certain lines in the spectra of a few stars to oxygen.

The March number of the *Astrophysical Journal*—which by reason of the diversity and untechnical character of its articles the general reader will find unusually interesting—closes with Bulletin No. 6 of the Yerkes Observatory, giving a description and reproduction of a photograph of the red of the spectrum of Saturn and Saturn's rings. The photograph shows clearly the absorption band at $\lambda$ 6183 in the spectrum of the planet and its absence in the spectrum of the rings; confirming previous visual observations of Vogel and Keeler, and showing conclusively that the rings possess none of the strong atmosphere of the planet.

## VARIABLE STARS.

J. A. PARKHURST.

### Maxima and Minima of Long Period Variables.

1899.

MAXIMA.

| | | Mag. | July |
|---|---|---|---|
| 678 | U Persei | 7.2 | 15 |
| 906 | R Trianguli | 5.8 | 11 |
| 1116 | X Ceti | 9.3 | 13 |
| 1386 | T Eridani | 7.2 | 16 |
| 2080 | R Columbae | 7.9 | 19 |
| 2404 | X Geminorum | 8 ? | 21 |
| 2684 | S Canis minoris | 7.2 | 15 |
| 3425 | X Hydrae | 8.4 | 8 |
| 4315 | R Comae | 7.4 | 15 |
| 4511 | T Ursae Majoris | 6.0 | 1 |
| 5037 | RR Virginis | 11 | 25 |
| 5338 | U Bootis | 9.1 | 18 |
| 5617 | U Librae | 9 | 27 |
| 5795 | W Scorpii | 10 | 12 |
| 5798 | RU Herculis | 8 | 18 |
| 6100 | RV Herculis | 9 | 3 |
| 6921 | S Sagittarii | 9.1 | 25 |
| 6943 | T Sagittae | 8.3 | 21 |
| 7162 | RS Aquilae | 10.0 | 16 |

MAXIMA, Con't.

| | | Mag. | July |
|---|---|---|---|
| 7192 | Z Cygni | 7.1 | 11 |
| 7502 | X Delphini | 8.0 | 6 |
| 7560 | R Vulpeculae | 7.5 | 1 |
| 8153 | R Lacertae | 8 3 | 17 |
| 8324 | V Cassiopae | 7.1 | 21 |
| 8369 | W Pegasi | 8.1 | 11 |

MINIMA.

| | | | |
|---|---|---|---|
| 243 | U Cassiopeae | <15 | 9 |
| 1805 | V Orionis | <13 | 14 |
| 4521 | R Virginis | 11 | 31 |
| 5430 | T Librae | <14.7 | 18 |
| 6207 | Z Ophiuchi | 12.5 | 25 |
| 6512 | T Herculis | 12.7 | 25 |
| 6900 | W Aquilae | 11.2 | 20 |
| 7468 | T Aquarii | 13 | 2 |
| 7733 | Y Capricorni | 14 ? | 1 |
| 8068 | S Lacertae | <12 | 10 |

**Minima of the Variable Stars of the Algol Type.**

[Given to the nearest hour in Greenwich Mean Time.]

1899.

U CEPHEI.

| | d | h |
|---|---|---|
| June | 4 | 12 |
| | 9 | 12 |
| | 14 | 11 |
| | 19 | 11 |
| | 24 | 11 |
| | 29 | 10 |

S CANCRI.

| | d | h |
|---|---|---|
| June | 13 | 16 |

S VELORUM.

| | d | h |
|---|---|---|
| June | 19 | 9 |

δ LIBRÆ.

| | d | h |
|---|---|---|
| June | 5 | 9 |
| | 7 | 16 |
| | 12 | 8 |
| | 14 | 16 |
| | 19 | 8 |
| | 21 | 16 |
| | 28 | 15 |

U CORONÆ.

| | d | h |
|---|---|---|
| June | 1 | 17 |
| | 8 | 15 |
| | 15 | 13 |
| | 22 | 10 |
| | 25 | 21 |

U OPHIUCHI.

Every 10th min.

P = 20h.13

| | d | h |
|---|---|---|
| June | 6 | 23 |
| | 15 | 8 |
| | 23 | 17 |

RS SAGITTARII.

| | d | h |
|---|---|---|
| June | 4 | 12 |
| | 6 | 22 |
| | 9 | 8 |
| | 11 | 18 |
| | 16 | 14 |
| | 21 | 10 |
| | 23 | 20 |
| | 28 | 16 |

W DELPHINI.

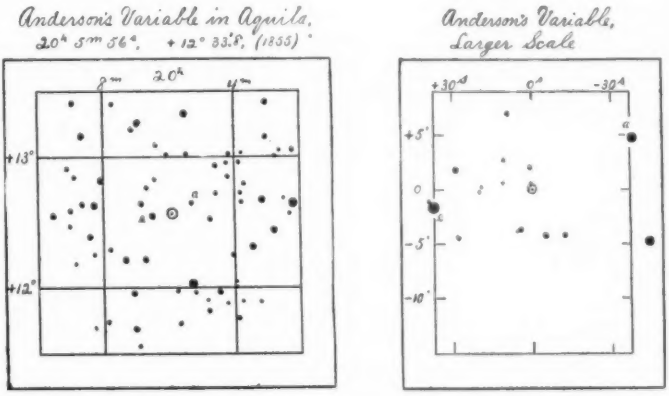
| | d | h |
|---|---|---|
| June | 3 | 10 |
| | 17 | 20 |
| | 22 | 16 |
| | 27 | 11 |

*DM* + 12°,3557

| | d | h |
|---|---|---|
| June | 1 | 15 |
| | 2 | 12 |
| | 3 | 10 |
| | 6 | 23 |
| | 7 | 20 |
| | 8 | 18 |
| | 9 | 15 |
| | 10 | 12 |
| | 11 | 10 |
| | 15 | 20 |
| | 16 | 18 |
| | 17 | 15 |
| | 18 | 13 |
| | 19 | 10 |
| | 23 | 21 |
| | 24 | 18 |
| | 25 | 15 |
| | 26 | 13 |
| | 27 | 10 |

*ANDERSON'S NEW VARIABLE IN AQUILAE.*—The charts presented this month will aid in identifying this variable, which is now near maximum. It was announced in No. 3520 of the *Nachrichten*, and a note in regard to it was inserted in POPULAR ASTRONOMY for November, 1898, page 532. After it was found here at about 12m.2, 1898 Oct 8, it slowly faded to a minimum not far from 13m about 1899, Jan. 1. In the meantime its position was found to be

R. A. 20h 5m 56s, Dec'l. +12° 33′.8, (1855)
8 3 41.7, (1900)




The conditions for measurement were unfavorable, the star being faint and rather low in the west, and the nearest catalogue stars quite distant; so that the

above position may need correction. Since its reappearance in the morning sky it has increased rapidly in brightness, being but little fainter than 8m, 1899; April 4. During the evening observations I found a 12m.7 star (r in the accompanying list) 0'.6 north of the star which I took to be the variable. On account of the unfavorable conditions it is just possible that this northern component is the variable, but this question can be settled in a few weeks.

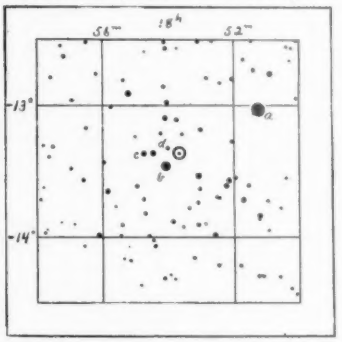
COMPARISON STARS.

| | Coor. from V. R. A. | Coor. from V. Dec. | Mag. | | Coor. from V. R. A. | Coor. from V. Dec. | Mag. |
|---|---|---|---|---|---|---|---|
| | m s | ′ | | | m s | ′ | |
| *b* | − 10.8 | − 44.2 | 9.9 | *n* | + 2.1 | + 8.5 | 7.0 11.1 |
| *a* | − 9.2 | − 37.6 | + 4.7 9.5 | *m* | + 2.6 | + 10.4 | + 2.7 12.4 |
| *d* | − 3.0 | − 12.4 | − 4.2 11.2 | *o* | + 2.7 | + 11.0 | + 0.6 12.8 |
| *c* | − 1.2 | − 5.1 | − 4.2 10.7 | *p* | + 4.6 | + 18.8 | 0.0 12.5 |
| *r* | 0.0 | + 0.2 | + 0.6 12.7 | *h* | + 6.8 | + 27.8 | − 4.4 10.4 |
| *l* | + 0.1 | + 0.5 | + 2.0 12.2 | *f* | + 7.0 | + 28.5 | + 1.8 10.5 |
| *g* | + 1.0 | + 3.9 | − 3.7 11.2 | *e* | + 9.0 | + 36.8 | − 1.7 8.6 |

*NEW STAR IN SAGITTARIUS.*—Elsewhere in this number will be found Harvard College Observatory Circular No. 42, giving account of the discovery of this new star by Mrs. Fleming, and details of the magnitudes derived from the photographs. The chart of the region, given here, is drawn from the catalogue and chart of Schönfeld's Southern Durchmusterung, and contains the stars from 5.5 to 10 magnitudes, within 1° of the new star. The position of the new star is—

FLEMING'S NEW STAR IN SAGITTARIUS.

R.A. 18h 53.7m, DEC. -13° 2.2 (1855)




| | | | |
|---|---|---|---|
| R. A | 18h | 53m 40s.1 | (1855) |
| | | 56 12 2 | (1900) |
| Decl. | − 13° | 21′ 50″ | (1855) |
| | | 18 16 | (1900) |

The field can perhaps be found without circles from the 5m.5 star marked *a* in the chart. The position of this star and the others referred to in the Harvard Circular are given in the following list, with the exception of − 16°.5283, which is too distant.

SCHONFELD'S DURCHMUSTERUNG, 1855. H. O. C.

| | ° | Mag. | R. A. h | m | s | Decl. ° | ′ | Mag. |
|---|---|---|---|---|---|---|---|---|
| *a* | − 13.5172 | 5.5 | 18 | 51 | 15.1 | − 13 | 1.7 | 5.28 |
| *d* | .5193 | 9 5 | | 54 | 1.1 | | 19.7 | 9.85 |
| *b* | .5194 | 7 8 | | 54 | 3 2 | | 27 7 | |
| *c* | − 13.5200 | 9 0 | 18 | 54 | 43.4 | − 13 | 21 8 | |

*U GEMINORUM.*—Following are all the observations I have received of the last maximum. They were made by Zaccheus Daniel, Wm. E. Sperra and J. A. Parkhurst.

| | Gr. T. | m | | Gr. T. | m | |
|---|---|---|---|---|---|---|
| 1899 | Feb. 27 | 9.24 | S | Mar. 7.54 | 13.3 | P |
| | 27 65 | 9.75 | D | 7.57 | 13.3 | D |
| | Mar. 5.62 | 12.7 | D | | | |

The maximum evidently occurred within a day or two of Feb. 27.

*SS CYGNI.*—The maximum of this star was determined quite definitely.

| | m | | | | m | |
|---|---|---|---|---|---|---|
| 1899 Mar. 4.96 | 11.3 | ± | P | Mar. 7.96 | 8 40 | P |
| 5.95 | 11.30 | | D | 12 96 | 9.83 | ± P |
| 6.97 | 10.90 | | P | 19.96 | 11.30 | P |

The time of passing $9^m.35$ on the rise is found by the curve to Mar. 7.5 Gr. Time, or 3.4 days earlier than the ephemeris based on the elements given in the March number, page 148. The maximum was a "short" one, following the order observed by the star since March 1897.

*POSITIONS OF NEW VARIABLES.*—In No. 3553 of the *Nachrichten* Dr. Ernest Hartwig, Director of the Remeis Observatory at Bamberg, Germany, has a valuable article on the positions and periods of 16 variables discovered within the last two years. Their places were measured with the 7 3-inch heliometer, which is one of the principal instruments of the Observatory, and exactly adapted to this work. He also gives his observations of SS Cygni from 1897, Sept. 24, to 1898, Jan. 31, which interval includes three maxima. At the close of the article Dr. Hartwig expresses the opinion that "the time intervals between the maxima show no regularity."

## PLANET NOTES FOR MAY.

H. C. WILSON

*Mercury*, having completed his retrograde movement in Pisces, is now moving eastward, following the Sun. He will be at greatest elongation, west from the Sun 26° 15′, May 9, and may be seen with the naked eye for a few mornings near that date, about an hour before sunrise.

*Venus* is the bright "morning star" seen toward the southeast. She will move through Pisces into Aries during this month. Her phase increases from 0.80 May 1 to 0.87 May 31, but her brightness decreases in the same time from 70 to 59.

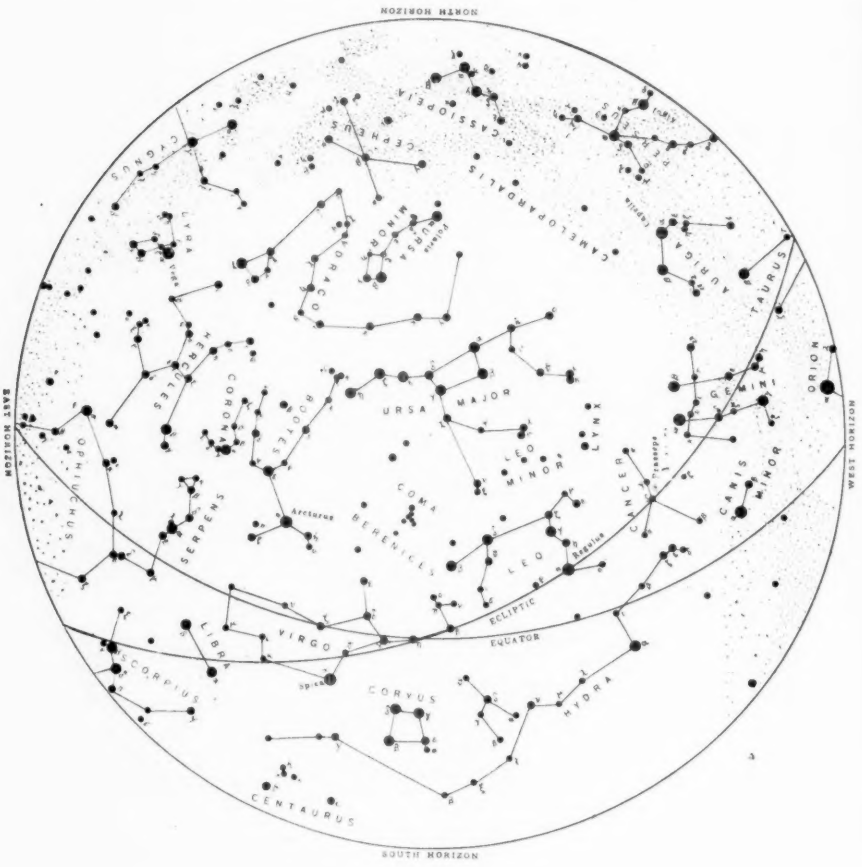
*Mars* has passed by the Praesepe cluster and is moving almost straight toward the star Regulus in Leo. The apparent disc of Mars is growing smaller day by day, with the increasing distance of the planet, and is now only 7″ in diameter, so that the markings upon it are seen with difficulty.

*Jupiter* is now in position to be observed in the evening at a convenient hour, being well up toward the meridian at 10 P. M. This should be the amateur's favorite planet, for the surface markings can be well seen with a telescope of only moderate power. Jupiter is moving very slowly westward between Libra and Virgo.

*Saturn* will be at opposition in June and may now be observed after midnight. This planet may be seen toward the southeast between the constellations Scorpio and Sagittarius. Its golden color distinguishes it from any of the stars in the vicinity. A very small telescope will show the presence of the rings, but the best views require something above 4 inches aperture.

We learn that the photographs upon which the new satellite of Saturn was discovered were taken by Dr. DeLisle Stewart, a former student at Goodsell Observatory, who is in charge of the work with the 24 inch Bruce photographic telescope at the Harvard Observatory station at Arequipa, Peru. The photographs were taken in August, 1898.

*Uranus* will be at opposition May 27, and may be best seen between three and four o'clock in the morning. It is in Scorpio, about five degrees north and a little west of Antares, and is moving slowly westward. It can be recognized by its greenish hued disc with a telescope of moderate power.




THE CONSTELLATIONS AT 9 P. M., MAY 1, 1899.

*Neptune* is too far to the west in the early evening for satisfactory observation.

## The Moon.

| | | Phases. | Rises. (Central Standard time at Northfield; Local Time 13m less.) | Sets. |
|---|---|---|---|---|
| | | | h m | h m |
| May | 2 | Last Quarter | 1 10 A. M. | 11 26 A. M. |
| | 9 | New Moon | 4 26 A. M. | 7 55 P. M. |
| | 17 | First Quarter | 11 48 A. M. | 1 8 A. M. |
| | 24 | Full Moon | 7 38 P. M. | 4 35 " |
| | 31 | Last Quarter | 12 11 A. M. | 11 47 " |

## Occultations Visible at Washington.

| Date 1899. | Star's Name. | Magnitude. | IMMERSION. Washington M. T. | IMMERSION. Angle f'm N pt. | EMERSION. Washington M. T. | EMERSION. Angle f'm N pt. | Duration. |
|---|---|---|---|---|---|---|---|
| | | | h m | ° | h m | ° | h m |
| May 12 | 1 Geminorum | 5.0 | 6 23 | 37 | 6 50 | 352 | 0 27 |
| 13 | ζ Geminorum | 4.0 | 10 8 | 129 | 10 58 | 263 | 0 50 |
| 18 | 55 Leonis | 6 2 | 9 56 | 98 | 11 4 | 323 | 1 8 |
| 18 | 57 Leonis | 6.9 | 10 35 | 144 | 11 40 | 275 | 1 5 |
| 25 | B. A. C. 5846 | 6.8 | 16 19 | 109 | 17 17 | 232 | 0 58 |
| 25 | 9 Ophiuchi | 3.3 | 16 38 | 140 | 17 10 | 201 | 0 32 |
| 30 | B. A. C. 7717 | 6.9 | 11 35 | 93 | 12 28 | 224 | 0 53 |

## Phenomena of Jupiter's Satellites.

Central Standard Time.

| | | h m | | |
|---|---|---|---|---|
| May | 2 | 10 3 P. M. | III | Oc. Dis. |
| | 3 | 12 11 A. M. | III | Ec. Re. |
| | | 2 34 " | II | Tr. In. |
| | | 2 54 " | II | Sh. In. |
| | | 4 49 " | II | Tr. Eg. |
| | 4 | 2 15 " | I | Tr. In. |
| | | 2 27 " | I | Sh. In. |
| | | 4 26 " | I | Tr. Eg. |
| | | 4 39 " | I | Sh. Eg. |
| | | 8 59 P. M. | II | Oc. Dis. |
| | | 11 30 " | I | Oc. Dis. |
| | | 11 42 " | II | Ec. Re. |
| | 5 | 1 53 A. M. | I | Ec. Re. |
| | | 8 41 P. M. | I | Tr. In. |
| | | 8 55 " | I | Sh. In. |
| | | 10 52 " | I | Tr. Eg. |
| | | 11 7 " | I | Sh. Eg. |
| | 6 | 8 21 " | I | Ec. Re. |
| | 10 | 1 17 A. M. | III | Oc. Dis. |
| | 11 | 11 16 P. M. | II | Oc. Dis. |
| | 12 | 1 14 A. M. | I | Oc. Dis. |
| | | 2 19 " | II | Ec. Re. |
| | | 3 47 " | I | Ec. Re. |
| | | 10 25 P. M. | I | Tr. In. |
| | | 10 50 " | I | Sh. In. |
| | 13 | 12 37 A. M. | I | Tr. Eg. |
| | | 1 2 " | II | Sh. Eg. |
| | | 6 45 P. M. | II | Sh. In. |
| | | 7 40 " | II | Oc. Dis. |
| | | 8 10 " | II | Tr. Eg. |
| | | 9 5 " | II | Sh. Eg. |
| | | 10 15 " | I | Ec. Re. |

| | | h m | | |
|---|---|---|---|---|
| May | 19 | 1 33 A. M. | II | Oc. Dis. |
| | | 2 59 " | I | Oc. Dis. |
| | 20 | 12 11 " | I | Tr. In. |
| | | 12 45 " | I | Sh. In. |
| | | 2 23 " | I | Tr. Eg. |
| | | 2 57 " | I | Sh. Eg. |
| | | 7 36 P. M. | III | Tr. Eg. |
| | | 8 9 " | II | Tr. In. |
| | | 8 18 " | III | Sh. In. |
| | | 9 19 " | II | Sh. In. |
| | | 9 25 " | I | Oc. Dis. |
| | | 10 5 " | III | Sh Eg. |
| | | 10 25 " | II | Tr. Eg. |
| | | 11 39 " | II | Sh. Eg. |
| | 21 | 12 9 " | II | Ec. Re. |
| | | 8 50 P. M. | I | Tr. Eg. |
| | | 9 26 " | I | Sh. Eg. |
| | 27 | 9 26 " | III | Tr. In. |
| | | 10 25 " | II | Tr. In. |
| | | 11 3 " | III | Tr. Eg. |
| | | 11 11 " | I | Oc. Dis. |
| | | 11 53 " | II | Sh. In. |
| | 28 | 12 17 A. M. | III | Sh. In. |
| | | 12 43 " | II | Tr. Eg. |
| | | 2 3 " | III | Sh. Eg. |
| | | 2 4 " | I | Ec. Re. |
| | | 8 24 P. M. | I | Tr. In. |
| | | 9 8 " | I | Sh. In. |
| | | 10 36 " | I | Tr. Eg. |
| | | 11 20 " | I | Sh. Eg. |
| | 29 | 8 32 " | I | Ec. Re. |
| | | 8 52 " | II | Ec. Re. |

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re', reappearance; Ec., eclispe; Oc., occultation; Tr., transit of satellite; Sh., transit of shadow.

## Jupiter's Satellites for May.

Central Standard Time.

*Phases of the Eclipses of the Satellites for an Inverting Telescope.*

I.

III.

II.

IV. No Eclipse.

*Configurations at 10h 30m for an Inverting Telescope.*

| Day. | West. | East. |
|---|---|---|
| 1 | ·3 ○ | 4· 1· ·2 |
| 2 | 4· ·1 ○ | 2· ·3● |
| 3 | 4· 2· ○ | 1· ·3 |
| 4 | 4· ·1 ○ | 3· ·2● |
| 5 | ○1· 4· ○ | 3· 2· |
| 6 | ·4 3 2· ○ | ·1 |
| 7 | ·4 3· ·2 1· ○ | |
| 8 | ·4 ·3 ○ | ·1 ·2 |
| 9 | ·4 ·1 ·3○ | 2· |
| 10 | 2· ○ | ·4 1· ·3 |
| 11 | ·1 ·2○ | ·4 3· |
| 12 | ○1· ○ | 3· 2· ·4 |
| 13 | 3· 2· ○ | ·1 ·4 |
| 14 | 3· ·2 1· ○ | 4· |
| 15 | ·3 ○ | ·1 ·2 4· |
| 16 | 1· ·3 ○ | 2· 4· |
| 17 | 2· ○ | 1· ·3 4· |
| 18 | 1· ·2 ○ 4· | ·3 |
| 19 | 4· ○ | 1· ·3 2· |
| 20 | ○2· 4· 3· ○ | ·1● |
| 21 | 4· 3· ·2 1· ○ | |
| 22 | 4· ·3 ○ | ·1 ·2 |
| 23 | ·4 1· ·3 ○ | 2· |
| 24 | ·4 2· ○ | 1· ·3 |
| 25 | ·4 ·1 ·2 ○ | ·3 |
| 26 | ·4 ○ | 1· ·2 3· |
| 27 | ○2· ○3· ·1 ○ | ·4 |
| 28 | ○1· 3· ·2 ○ | ·4 |
| 29 | ·3 ○ | ·2 ·1 ·4 |
| 30 | ·3 1· ○ | 2· ·4 |
| 31 | 2· ○ | ·3 ·1 4· |

## COMET NOTES.

H. C. WILSON.

### Comets of the Year 1898.

| Designation. | Discoverer. | Date. | Periodic Comet. |
|---|---|---|---|
| 1898 I = *h* 1898 | Perrine | Mar. 19 | |
| 1898 II = *a* 1898 | Perrine | Jan. 2 | Winnecke's |
| 1898 III = *d* 1898 | Tebbutt | June 11 | Encke's |
| 1898 IV = *f* 1898 | Hussey | June 16 | Wolf's |
| 1898 V = *g* 1898 | Giacobini | June 18 | |
| 1898 VI = *e* 1898 | Perrine | June 14 | |
| 1898 VII = *c* 1898 | Coddington | June 11 | |
| 1898 VIII = *j* 1898 | Chase | Nov. 14 | |
| 1898 IX = *b* 1898 | Perrine | Sept. 12 | |
| 1898 X = *i* 1898 | Brooks | Oct. 20 | |

APPROXIMATE ELEMENTS.

| Perihelion. | Ω | ϖ | *i* | *q* | *e* |
|---|---|---|---|---|---|
| | ° ′ | ° ′ | ° ′ | | |
| I 1898 Mar. 17.36 | 262 32 | 145 4 | 72 27 | 1.0985 | |
| II Mar. 20.39 | 100 52 | 173 21 | 17 0 | 0.9241 | 0.7147 |
| III May 27.80 | 334 47 | 183 59 | 12 55 | 0 3407 | 0.8464 |
| IV July 4.60 | 206 27 | 172 52 | 25 12 | 1.6035 | 0.5553 |
| V July 25.51 | 278 17 | 22 25 | 166 51 | 1.5013 | |
| VI Aug. 16.20 | 269 6 | 205 36 | 70 2 | 0.6265 | |
| VII Sept. 14.05 | 73 59 | 233 16 | 69 56 | 1.7015 | |
| VIII Sept. 20.15 | 95 51 | 4 38 | 22 30 | 2.2693 | |
| IX Oct. 20.53 | 34 56 | 162 26 | 28 51 | 0.4195 | |
| X Nov. 23.16 | 96 20 | 123 34 | 140 21 | 0.7560 | |

### Epemeris of Comet 1898 VIII (Chase).

[By E. F. Coddington in *Astronomische Nachrichten* No. 3555].

| Greenwich Midn. | R. A. | Decl. | log Δ | Br. |
|---|---|---|---|---|
| | h m s | ° ′ ″ | | |
| May 1 | 10 49 4.2 | + 34 34 5 | 0.4644 | 0.28 |
| 3 | 49 55.3 | 34 17 56 | | |
| 5 | 50 49.9 | 34 1 30 | 0.4755 | 0.26 |
| 7 | 51 47.8 | 33 44 48 | | |
| 9 | 52 49.1 | 33 27 53 | 0.4866 | 0.25 |
| 11 | 53 53.6 | 33 10 44 | | |
| 13 | 55 1.2 | 32 53 23 | 0.4975 | 0.23 |
| 15 | 56 11.8 | 32 35 50 | | |
| 17 | 57 25.2 | 32 18 8 | 0.5083 | 0.22 |
| 19 | 10 58 41.3 | 32 0 17 | | |
| 21 | 11 0 0.0 | 31 42 18 | 0.5187 | 0.20 |
| 23 | 1 21.3 | 31 24 12 | | |
| 25 | 2 44.9 | 31 5 58 | 0.5293 | 0.19 |
| 27 | 4 10.8 | 30 47 39 | | |
| 29 | 5 38.9 | 30 29 14 | 0.5395 | 0.18 |
| 31 | 11 7 9.0 | + 30 10 45 | | |

**Elements and Ephemeris of Comet Tempel$_2$ (1873 II).**—In *Astronomische Nachrichten* No. 3554, Dr. L. Schulhof, of the Paris Observatory, gives an ephemeris of this periodic comet, in which he thinks the uncertainty does not exceed ± 30ˢ in R.A. and ± 3′ in Decl. in May. The perturbations by Jupiter between 1894 and 1899 have been considerable and have retarded the perihelion

passage more than 15 days. The comet should be considerably brighter in May than when it was discovered by Finlay in 1894.

ELEMENTS.

Epoch of Osculation 1899 June 18.0 Paris M. T.

$M = 352^\circ\ 26'\ 22''.9$
$\pi = 306\ 33\ 51\ .5$
$\Omega = 120\ 57\ 57\ .0$ } 1899.0
$i = 12\ 38\ 56\ .2$
$\varphi = 32\ 49\ 41.2$
$\mu = 671''.8846$
$\log a = 0.481808$

EPHEMERIS OF COMET TEMPEL$_2$ (1873 II.)

| | App. R. A. | | | App. Decl. | | | $\log \Delta$ | Ab.-Time | | $1:r^2\Delta^2$. |
|---|---|---|---|---|---|---|---|---|---|---|
| | h | m | s | ° | ′ | ″ | | m | s | |
| May 1 | 18 | 43 | 47.6 | − 4 | 55 | 11 | 9.96822 | 7 | 43 | 0.418 |
| 2 | | 45 | 31.1 | 4 | 50 | 46 | 9.96284 | | | |
| 3 | | 47 | 14.2 | 4 | 46 | 25 | 9.95744 | | | |
| 4 | | 48 | 57.0 | 4 | 42 | 8 | 9.95202 | 7 | 26 | 0.460 |
| 5 | | 50 | 39.5 | 4 | 37 | 57 | 9.94659 | | | |
| 6 | | 52 | 21.5 | 4 | 33 | 51 | 9.94114 | | | |
| 7 | | 54 | 3.2 | 4 | 29 | 51 | 9.93567 | | | |
| 8 | | 55 | 44.5 | 4 | 25 | 58 | 9.93018 | 7 | 4 | 0.521 |
| 9 | | 57 | 25.5 | 4 | 22 | 13 | 9.92467 | | | |
| 10 | 18 | 59 | 6.0 | 4 | 18 | 33 | 9.91915 | | | |
| 11 | 19 | 0 | 46.1 | 4 | 15 | 3 | 9.91362 | | | |
| 12 | | 2 | 25.8 | 4 | 11 | 42 | 9.90807 | 6 | 43 | 0.592 |
| 13 | | 4 | 5.2 | 4 | 8 | 30 | 9.90251 | | | |
| 14 | | 5 | 44.2 | 4 | 5 | 29 | 9.89693 | | | |
| 15 | | 7 | 22.7 | 4 | 2 | 38 | 9.89134 | | | |
| 16 | | 9 | 0.9 | 3 | 59 | 59 | 9.88574 | 6 | 23 | 0.673 |
| 17 | | 10 | 38.7 | 3 | 57 | 31 | 9.88012 | | | |
| 18 | | 12 | 16.1 | 3 | 55 | 17 | 9.87450 | | | |
| 19 | | 13 | 53.1 | 3 | 53 | 15 | 9.86886 | | | |
| 20 | | 15 | 29.7 | 3 | 51 | 28 | 9.86322 | 6 | 4 | 0.764 |
| 21 | | 17 | 6.0 | 3 | 49 | 55 | 9.85756 | | | |
| 22 | | 18 | 41.8 | 3 | 48 | 38 | 9.85190 | | | |
| 23 | | 20 | 17.3 | 3 | 47 | 37 | 9.84623 | | | |
| 24 | | 21 | 52.3 | 3 | 46 | 53 | 9.84056 | 5 | 45 | 0.869 |
| 25 | | 23 | 27.0 | 3 | 46 | 27 | 9.83488 | | | |
| 26 | | 25 | 1.2 | 3 | 46 | 19 | 9.82919 | | | |
| 27 | | 26 | 35.0 | 3 | 46 | 30 | 9.82350 | | | |
| 28 | | 28 | 8.4 | 3 | 47 | 2 | 9.81781 | 5 | 28 | 0.987 |
| 29 | | 29 | 41.4 | 3 | 47 | 55 | 9.81212 | | | |
| 30 | | 31 | 13.8 | 3 | 49 | 10 | 9.80642 | | | |
| 31 | | 32 | 45.8 | 3 | 50 | 48 | 9.80072 | | | |
| June 1 | 19 | 34 | 17.4 | − 3 | 52 | 50 | 9.79502 | 5 | 11 | 1.121 |

**Corrected Ephemeris of Tuttle's Comet 1899** *b*.—In *Astronomische Nachrichten*, No. 3555, Mr. J. Rahts gives a corrected ephemeris of Tuttle's comet which satisfies closely the early observations of this year. The elements are

Epoch 1899, May 4.5, Berlin M. T.

$M = 359^\circ\ 59'\ 46''.7$
$\pi = 116\ 29\ 3\ .0$
$\Omega = 269\ 49\ 53\ .6$ } 1900.0
$i = 54\ 29\ 16\ .3$
$\varphi = 55\ 15\ 23\ .7$
$\mu = 259''.6234$

EPHEMERIS OF TUTTLE'S COMET.

| Berlin Midn. | App. R. A. h m s | App. Decl. ° ′ ″ | log Δ | Ab.-Time m s | Br' |
|---|---|---|---|---|---|
| April 30 | 4 43 13.3 | + 12 2 56 | 0.23654 | 14 18.2 | 1.89 |
| May 1 | 46 38.9 | 11 34 21 | | | |
| 2 | 50 4.0 | 11 5 35 | | | |
| 3 | 53 28.8 | 10 36 39 | | | |
| 4 | 4 56 53.1 | 10 7 33 | 0.23576 | 14 16.7 | 1.90 |
| 5 | 5 0 16.9 | 9 38 18 | | | |
| 6 | 3 40.4 | 9 8 53 | | | |
| 7 | 7 3.6 | 8 39 20 | | | |
| 8 | 10 26.4 | 8 9 38 | 0.23502 | 14 15.2 | 1.91 |
| 9 | 13 49.0 | 7 39 47 | | | |
| 10 | 17 11.3 | 7 9 49 | | | |
| 11 | 20 33.4 | 6 39 44 | | | |
| 12 | 23 55.3 | 6 9 32 | 0.23432 | 14 13.8 | 1.88 |
| 13 | 27 17.0 | 5 39 14 | | | |
| 14 | 30 38.6 | 5 8 50 | | | |
| 15 | 34 0.1 | 4 38 20 | | | |
| 16 | 37 21.5 | 4 7 45 | 0.23368 | 14 12.6 | 1.86 |
| 17 | 40 42.8 | 3 37 4 | | | |
| 18 | 44 4.1 | 3 6 18 | | | |
| 19 | 47 25.3 | 2 35 28 | | | |
| 20 | 50 46.6 | 2 4 34 | 0.23315 | 14 11.5 | 1.82 |
| 21 | 54 8.0 | 1 33 37 | | | |
| 22 | 5 57 29.5 | 1 2 36 | | | |
| 23 | 6 0 51.0 | 0 31 32 | | | |
| 24 | 4 12.7 | + 0 0 25 | 0.23279 | 14 10.8 | 1.77 |
| 25 | 7 34.7 | − 0 30 44 | | | |
| 26 | 10 56.9 | 1 1 54 | | | |
| 27 | 14 19.4 | 1 33 6 | | | |
| 28 | 17 42.2 | 2 4 19 | 0.23266 | 14 10.6 | 1.71 |
| 29 | 21 5.3 | 2 35 33 | | | |
| 30 | 24 28.8 | 3 6 46 | | | |
| 31 | 6 27 52.7 | + 3 38 0 | | | |

**Elements and Ephemeris of Comet Holmes 1892 III.**—In *Astronomische Nachrichten*, No. 3553, Mr. H. J. Zwiers gives a search ephemeris for this periodic comet, extending from April 26 to July 31, 1899. The brightness should be from two to four times that of the comet when last seen in 1893. The comet will not be very favorably situated for northern obsesvers until June. It is to be looked for in the morning from one to two hours before sunrise.

ELEMENTS.

Epoch and Osculation 1899 Sept. 9 0 Greenwich M. T.

$M = 19^\circ\ 15'\ 53''.535$
$\pi = 345\ 47\ 52.94$ } 1899.0
$\Omega = 331\ 43\ 31.97$ } 1899.0
$i = 20\ 48\ 9.91$ } 1899.0
$\varphi = 24\ 17\ 21.86$
$\mu = 516''.2734$
$\log a = 0.558085$
$T = 1899$ April 27.6651 Greenwich M. T.

Mr. Zwiers says that the probable error of the period of revolution is only ± 0.7 days, but gives three ephemerides assuming the perihelion passage to vary ± 8 days. This indicates to the observer each day a narrow space 3° long in R. A. and ranging about 2° in Decl. within which the comet is to be searched for, with the probability that it is to be found near the center of the area indicated. We give here the most probable of three ephemerides.

SEARCH EPHEMERIS FOR HOLMES' COMET (1892 III )

| 1899. | R. A. | Decl. | $1 : r^2\Delta^2$ |
|---|---|---|---|
| | h m s | ° ′ ″ | |
| May 2 | 0 6 35.7 | + 5 14 44 | 0.0277 |
| 4 | 10 10.1 | 5 52 1 | 280 |
| 6 | 13 43.8 | 6 29 19 | 282 |
| 8 | 17 17.0 | 7 6 37 | 285 |
| 10 | 20 49.5 | 7 43 54 | 287 |
| 12 | 24 21.4 | 8 21 11 | 290 |
| 14 | 27 52.7 | 8 58 25 | 292 |
| 16 | 31 23.3 | 9 35 39 | 295 |
| 18 | 34 53.2 | 10 12 50 | 298 |
| 20 | 38 22.5 | 10 50 0 | 301 |
| 22 | 41 51.1 | 11 27 8 | 304 |
| 24 | 45 19.1 | 12 4 13 | 306 |
| 26 | 48 46.2 | 12 41 15 | 309 |
| 28 | 52 13.1 | 13 18 14 | 312 |
| 30 | 55 39.1 | 13 55 8 | 315 |
| June 1 | 0 59 4.4 | + 14 31 58 | 0.0318 |

## Ephemeris of Swift's Comet *a* 1899.

[From *Astronomische Nachrichten* No. 3536.]

| Berlin Midnight | R. A. | Decl. | log. $r$ | log. $\Delta$ | Br. |
|---|---|---|---|---|---|
| | h m s | ° ′ | | | |
| May 1 | 0 8 37 | + 22 33.3 | 9.7861 | 0.0001 | 1.81 |
| 2 | 5 5 | 23 26.0 | | | |
| 3 | 0 1 28 | 24 20.9 | 9.8154 | 9.9796 | 1.74 |
| 4 | 23 57 45 | 25 17.9 | | | |
| 5 | 53 54 | 26 17.3 | 9.8428 | 9.9581 | 1.69 |
| 6 | 49 53 | 27 19.3 | | | |
| 7 | 45 41 | 28 24.0 | 9.8685 | 9.9359 | 1.67 |
| 8 | 41 17 | 29 31.5 | | | |
| 9 | 36 40 | 30 42.0 | 9.8926 | 9.9129 | 1.66 |
| 10 | 3 45 | 31 55.7 | | | |
| 11 | 26 28 | 33 12.7 | 9.9153 | 9.8894 | 1.66 |
| 12 | 20 48 | 34 33.1 | | | |
| 13 | 14 41 | 35 56.8 | 9.9366 | 9.8656 | 1.68 |
| 14 | 8 2 | 37 23.9 | | | |
| 15 | 23 0 46 | 38 54.2 | 9.9568 | 9.8420 | 1.71 |
| 16 | 22 52 48 | 40 27.8 | | | |
| 17 | 44 2 | 42 4.2 | 9.9759 | 9.8189 | 1.74 |
| 18 | 34 21 | 43 42.9 | | | |
| 19 | 23 37 | 45 23.1 | 9.9940 | 9.7971 | 1.77 |
| 20 | 22 11 40 | 47 4.2 | | | |
| 21 | 21 58 20 | 48 44.5 | 0.0113 | 9.7773 | 1.79 |
| 22 | 43 29 | 50 22.7 | | | |
| 23 | 26 59 | 51 56.5 | 0.0276 | 9.7606 | 1.79 |
| 24 | 21 8 40 | 53 23.9 | | | |
| 25 | 20 48 31 | 54 41.8 | 0.0433 | 9.7480 | 1.77 |
| 26 | 26 33 | 55 47.5 | | | |
| 27 | 20 3 7 | 56 38.4 | 0.0582 | 9.7403 | 1.71 |
| 28 | 19 38 21 | 57 11.4 | | | |
| 29 | 18 12 56 | 57 26.1 | 0.0725 | 9.7383 | 1.61 |
| 30 | 18 47 26 | 57 20.2 | | | |
| 31 | 18 22 26 | 56 85.6 | 0.0862 | 9.7423 | 1.49 |

## GENERAL NOTES.

It is the purpose of this Journal to allow authors of new matter not manifestly erroneous to present their views, without thereby incurring the responsibility of full editorial sanction. This custom is now so well established in scientific journals, that it scarcely needs a reference of this kind. In spite of usual precaution, however, articles will sometimes appear that contain serious errors which were not noticed at the time. In such cases, however, we are sure to hear from it soon, and in a way to set the matter right. The leader of this issue is an example. As good a mathematician as Dr. See is we wonder that he should have been so much in error, in what appeared to be a carefully written article on an important theme. We wonder also that excellent mathematicians in this country should have given the article approval if they knew it contained serious errors. It is probable that the authorities have erred as we did, in not giving the matter the full attention it deserved in the outset.

**Total Solar Eclipse of May 18, 1900.**—In a late number of *Science*, Professor T. H. Safford, of Williams College, speaks of the need of early preparation for the coming total solar eclipse in May, 1900. He emphasizes the idea that work may be done on both sides of the Atlantic, on one and the same plan with advantage, and specifies the points of Norfolk, Va., and Ovár, Portugal, as places, at which the time of totality would be long enough to carry out such a plan, especially in regard to photography. Such suggestions are timely. It would be well if many experienced astronomers would give in the same way their views of what may be done, and how to do it.

**Dr. Herman Davis.**—We learn that Dr. Herman S. Davis, who for the past five years has been on the staff of the Observatory of Columbia University in New York, has resigned his position. He is designing now to devote his time more assiduously to completing the new reduction of Piazzi's star observations. This work was begun two or three years ago and we would judge from an informal report made at the last meeting of the *Astronomische Gesellschaft* (V. J. S. XXXIII: 279-83) and from occasional letters that much progress is being made on the computations. From 1792 to 1813 Piazzi made over 125,000 observations with his meridian circle and transit instruments, and when these are reduced with the best modern constants and methods a valuable catalogue of about 8,000 stars for the epoch 1800 ought to result.

Professor Porro and Dr. Balbi, of the Turin Observatory, Italy, are reducing the 34,000 observations of right ascension made with the Transit in co-operation with Dr. Davis who has undertaken all the work of the 91,000 observations of right ascension and declination made with the Circle. Only a young man of great enthusiasm would begin so extensive a task as this when necessarily relying upon his own resources of time and energy. But while that was the case when his decision was first made, yet we are informed that he has been very fortunate in securing since then abundant co-operation. One investigation connected with the work is being done by Miss Flora Harpham, a former student at this Observatory, but now chief-computer at Columbia University. Professor Cunningham, Director of the Observatory of Swarthmore College, is co-operating with Miss Harpham (See POPULAR ASTRONOMY, VI: 222).

We learn also that all the Piazzi stars are being again observed for the

epoch 1900, to be used by Dr. Davis in this work, in addition to which Dr. Gill, of the Cape of Good Hope, has volunteered to make such observations of the southern stars as may be better done from that location than from the northern hemisphere.

Two computers have been employed during a portion of this year with funds furnished by Miss Catherine W. Bruce, who has so generously aided many branches of astronomy and who has thus added one more benefaction to the many for which all astronomers are grateful.

We congratulate Dr. Davis upon the progress of his work and wish him success in its continuation.

---

**The Gegenschein.**—There were so many poor prints of the plate accompanying Professor Barnard's article last month that the plate is reproduced in this issue.

---

**William Pick's Constellations** is the title of a book unknown to us. Can any one give information that will enable us to secure a copy?

---

**David Gill**, of Cape of Good Hope was awarded, April 4, 1899, the fifth Watson Medal by the National Academy of Sciences, for honorable work in perfecting the application of the heliometer.

---

**Bright Leonids, 1898**.—Dec. 11th.—Leonid with well-defined streak; seen southeast.

Dec. 14th.—At 6:15 A. M. Shortly before sunrise a meteor passed from the zenith to the southwest; it was extremely brilliant in the early morning and two witnesses had their attention attracted to it by the rushing sound accompanying its transit.

1899. Jan. 16.—At 6:29 P. M. a large meteor traversed the southern sky. When disruption occurred two distinct pieces were seen, one about the brilliancy of a first magnitude star and the other about that of one of the third magnitude.

Feb. 20th.—Very bright Leonid seen near midnight. It made a shadow, so dazzling was the meteor.

Feb. 21st.—A Leonid leaving a gorgeously colored trail seen in the east at 7:15 P. M.

Apr. 3rd.—At 9:07 P. M. a rocket-like meteor was seen several degrees below Capella in the northwest and it disappeared far to the southwest, being distinctly visible for about 70°. The heavenly wanderer wavered just like a large rocket and the train was very bright. C. N. FOUGHT.

---

At the conference of astronomers and astrophysicists, held at the Harvard College Observatory in August last, a committee consisting of Simon Newcomb, George E. Hale, E. E. Barnard and W. W. Campbell was appointed to gather information regarding intended observations of the total eclipse of the Sun which will occur on May 28, 1900. As the Moon's shadow will pass from near New Orleans to near Norfolk, it may be expected that many observers will take part in the eclipse work, and the committee is ready to do anything in its power to secure concert of action among them.

It is well to remark, in the first place, that the duration of totality is brief. It will amount only to about $1^m\,40^s$ where the shadow passes into the Atlantic

Ocean near Norfolk, and will diminish toward the southwest. Hence less can be done than if a longer duration were available. Observers will probably prefer stations east of the Alleghenies, as west of those mountains the duration will range from $1^m\ 13^s$ near New Orleans to $1^m\ 30^s$ near the mountains. The committee are seeking information in regard to

(1) What measure, if any, should be taken to secure observations of the eclipse on a well-concerted plan;

(2) As to what classes of observations are to be considered most important, with suggestions regarding the best means of making them.

(3) What parties from Colleges expect to take part in the work in question.

(4) Whether such parties can raise the necessary funds for the work desired to undertake, or consider some general appeal to the public to be required.

(5) What class of observations such party is best prepared to make, or will be likely to prosecute.

Replies, which the committee will be glad to have permission to publish, should this prove to be desirable, may be addressed to the Secretary, Yerkes Observatory, Williams Bay, Wis.

---

**The Sun-Dial.**—Mr. Saunder's article in the April POPULAR ASTRONOMY very thoroughly disposes of any retrogression of the shadow cast by a stylus directed towards the pole. But I must take exception to his theory that the phenomenon was due to the shadow of the vertical "termination" of the stylus, for the simple reason that that would never have been watched. And in any event the assigning of such causes seems to me to be quite futile. They all go on the assumption that the dial was newly fixed in position for else the variation would have been familiar to all. If, as seems probable, the Ahaz in question was the father of Hezekiah a comparison of the date of his death with that of Hezekiah's sickness will invalidate this. Moreover, to admit of any retrogression, the stylus would have to be fixed at such an angle as would render it worthless as a time-keeper except with the aid of carefully computed and complicated curves, which themselves would show that the retrogression at certain periods was about to take place. And finally, unless the dial were immediately destroyed, the regular retrogression of the shadow at the same season would effectually dispel any idea that the miraculous had taken place.

UNIVERSITY OF IOWA, April 13, 1899. BURTON S. EASTON.

---

**A New Star in Sagittarius**—From an examination of the Draper Memorial photographs, Mrs. Fleming has discovered a new star in the constellation Sagittarius. Its position for 1900 is R. A. $= 18^h\ 56^m.2$, Dec. $= -13°18'$. It was too faint to be photographed on eighty plates taken between October 18, 1888, and October 27, 1897, although stars as faint as the fifteenth magnitude appear on some of them. It appears on eight photographs taken while it was bright. On March 8, 1898, it was of the fifth magnitude, and on April 29, 1898, of the eighth magnitude. A plate taken this morning, March 9, 1899, shows that the star is still visible and is of the tenth magnitude. Two photographs show that its spectrum resembles those of other new stars. Fourteen bright lines are shown, six of them due to hydrogen. The entire number of new stars discovered since 1885 is six, of which five have been found by Mrs. Fleming.

E. C. PICKERING.

HARVARD COLLEGE OBSERVATORY, CAMBRIDGE, Mass.
March 9, 1899.

**The Recent Cyclonic Sunspot.**—In the beginning of March the solar axis inclines 7° 15′ from the pole of the ecliptic, and consequently from an earthly standpoint the solar equator has an equal amount of seeming convexity towards the north pole of the sun. The ascent and descent of a spot in the course of rotation produces a slightly cyclonic effect the value of which would be only about 14° 30′; but the following extract from a record of daily observations of the solar disk. Such vortex motion is occasionally detected, and in this case accords with M. Faye's theory of spot formation which demands that it should take place in the direction of the hands of a watch when the spot is south of the solar equator. As the earth's motion around the sun causes the summit of the convexity to appear on the east side of the disk in April, the measurement of spot positions is especially difficult, and it was only ascertained that this remarkable spot was between 9° and 14° south heliographic latitude.

EXTRACT FROM RECORD OF SUNSPOTS WITH 4 INCH TELESCOPE.

March 26th, 1899. There is a spot of more than average size inside the E. limb The group of the 17th inst. is still visible inside the west limb, and the small spot of the 20th is central, thus there are 3 distinct markings on the disk. The new spot is oval in a north and south direction.

27th. The spot of yesterday is now the only one discernible.

28th. It seems to be about south lat. 14°.

29th. It is longest in an east and west direction.

30th. The spot though slightly foreshortened measures fully 20,000 miles in an east and west direction.

31st. It is still oval. There is no other spot on the disk.

April 1st. There are a few very small but distinct spots extending northward from the eastern side of the penumbra.

2nd. The spot seems to lie between 9 and 14 degrees south latitude. The stream of small spots are gone.

3rd. The spot was observed through a light layer of cloud. No other spot is visible.

4th. The south side of the umbræ is slightly concave on its south edge and the position is more perpendicular.

5th. It is still more perpendicular in the foreshortened view, and the change of position seems more than that due to the upward curve of the solar equator. It has veered round about 45 degrees in the direction of the hands of a watch.

6th. It is nearing the west limb.

7th. It is still visible foreshortened. There are no other spots on the disk.

8th. It is no longer discernible. The disk is unspotted.

ROSE O'HALLORAN.

SAN FRANCISCO, April, 1899.

---

**The Planet Eros.**—The god of Love has not yet been once more placed in the astronomical sky. Monsieur Witt gave the name Eros to the curious little planet which he had discovered, number 433, which happens to be, as we have seen it, nearer the earth since its medium distance is less than that of Mars.

The nomenclature of small planets had limited itself to feminine names: as Ceres, Pallas, Juno, Vesta, Astrea, Hebe, Iris, Flora, etc.

Eros and Love, it is said are synonymous. Is it more masculine than feminine? The question is not astronomical, and we will not discuss it here.

However the christening has lately been accepted as a happy passing between goddesses and gods, and it will be possible in the future to give to small planets names of gods, although the feminine mythology is yet far from being exhausted.

---

**Munich Observatory.**—The third volume of the "*Neue Annalen der K. Sternwarte in Munchen,*" has just come to hand. It is edited by Dr. H. Seeliger, Director of the Observatory and contains four important investigations:

1. Investigation of the Periodic Comet 1889 V (Brooks), by Dr. J. Bauschinger.
2. Investigation of the atmospheric refraction, by Dr. J. Bauschinger.
3. Poincaré's theory of the equilibrium of a homogeneous rotating fluid mass, by Dr. K. Schwarzschild.
4. The rotation period of the planet Venus, with an appendix containing observations of the surface markings of the planets Venus and Mercury, with 9 plates, by Dr. W. Villiger.

The first three have been previously published in separate pamphlets. In the last the author discusses the available observations of the surface markings of Venus, together with those made at Münich. He finds it impossible to determine a reliable value of the rotation period. An important part of the investigation is a study of the physiological effect of light coming from a partially illuminated sphere. The writer shows that the dark meridional streaks, which have been depended upon by observers to prove the slowness of the rotation of Venus, can be quite perfectly explained as physiological effects, and that they may wholly mask the real surface markings which are mingled with them in the image of the planet.

---

## BOOK NOTICES.

**Corona and Coronet Days. By Mabel Loomis Todd.**—As Mrs. Todd very truly remarks in the introduction to her book, "chasing eclipses, always of interest in itself whether the eclipse be caught or not, yields great wealth to science when those elusive phenomena are properly overtaken." In the Corona and Coronet, Mrs. Todd describes in a most entertaining way, the Amherst Eclipse Expedition to Japan, in Mr. James' schooner yacht Coronet, to observe the total eclipse of the Sun, August 9th, 1896. Of late years research on the corona has been mainly by means of photography, the telescope being transformed into a camera. Professor Todd devised the following methods for obtaining photographs of the corona. "Exhaust air currents through pneumatic tubes, connected with each telescope and plate-holder were controlled by a slowly moving perforated sheet of paper, similar to those now familiar in automatic musical instruments. Movements of absolute precision allowed the exposure of over three hundred plates during the period of totality. The apparatus proving somewhat bulky, electricity was made the controlling power when in 1895 plans were maturing for Yezo. Endless chains for plate-holders of different sizes were allowed to pass before each of the twenty telescopes, at varying rates of speed. A copper cylinder full of pins revolved slowly, each pin as it passed along touching its appropriate metal tooth, and closing a circuit that set in motion some particular instrument at any prearranged instant during the two minutes and forty seconds while totality should prevail. A moderate calcula-

tion of its capacity showed that four hundred pictures could be taken, the movement of each exposing shutter and its corresponding plate-chain being deliberate and precise." By means of clock-work the telescopes remained constantly pointed at the Sun.

The morning of the eclipse dawned through a heavy shower, succeeded by sunshine, and at one o'clock almost half the sky was blue. By two o'clock, the Sun was partially obscured by a dimly drifting mass of cloud, but as the precious moments passed on, the corona gleamed through the mist. "As the beautiful corona lay there in the clouds, a soft unearthly radiance, the poetic effect as strong as if in a clear sky, the scientific value lost in vapors, it was still noticeably flattened at the poles and extended equatorially, and must have been of unusual brilliance to show so distinctly through cloud. The shape gives suggestion to astronomers as to new lines of future research."

A few pictures of the blurred corona were taken, although of little practical use. But a practical demonstration was given of the great number of instruments that can be employed in recording the corona automatically, thus lengthening the precious moments of totality.

Mrs. Todd has woven a charm of romance around the scientific details of the expedition, and in a bright, sympathetic way has described the sea-voyage from San Francisco to Japan, the peculiarities of the natives of Hawaii, and the strange people of Ainu land, and the descriptions of their home. The book is deeply interesting, not only as an excellent record of an eclipse expedition, giving one a glimpse of the difficulties that beset the way of an astronomer in pursuit of a shadow, but it also contains a vivacious account of the scenery, customs and people of strange lands observed during this remarkable cruise. The book is a success in every way, and is one of the most delightful books on travel lately published. M. P.

## PUBLISHER'S NOTICES.

**Contributors** are asked to prepare copy carefully, and to write *all proper names very plainly*. If other language than the English is used to any considerable extent it should be type-written. Manuscript to be returned should be accompanied by postage for that purpose.

**All Drawings** for publication should be done in India ink, twice the size that the cut will be on the printed page. The lines, figures and letters should be made even, very smooth and uniformly black in every part of the copy, in order to secure the best reproductions possible by the modern quick processes of engraving now most generally used.

**Proofs** will generally be sent to authors living in the United States, if copy is furnished before the tenth of the month preceding that of publication. We greatly prefer that authors should read their own proofs, and we will faithfully see that all corrections are made in the final proofs.

**Renewals.**—Notices of expiration of subscription will hereafter be sent with the last number of this publication for which payment has been made. It is especially requested that subscribers fill out the blank notice sent them and promptly return it to the publisher, as this publication will not be continued beyond the time for which it has been ordered.

All correspondenceand all remittances should be sent to

WM. W. PAYNE,
Northfield, Minn., U. S. A.